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OUR KNOWLEDGE OF ATOMIC NUCLEI.*

BY

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The subject which within the past few years has come to be known as "nuclear physics" is an outgrowth and in many ways the culmination of the research in atomic physics which began with the opening of the present century. You are all familiar with many of the practical results of this earlier work, though possibly you do not all realize how directly most of our recent technical developments are due to what at first sight seems like a highly impractical and literally intangible field of investigation. In consequence, I would like first to consider briefly why we study atoms and molecules and what evidence the pure scientist can offer the engineers, who are the applied scientists, and also all the other non-technical members of society to justify their continued support of pure science. For without this support we cannot continue to make fundamental advances in our knowledge of, and control over, our physical environment.

The motivations of the pure scientist would appear to many at first thought as whimsical and abstract as the immediate results he achieves. The briefest explanation of why he works is curiosity rather than the necessity of earning a livelihood. The questions he asks are "why?" and "how?",

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rather than "what good will come of all this?" The latter is the first query that occurs to most people and is a question that some of us must answer if the aims and results of pure science are to be widely understood. The gap between these points of view is bridged by realizing that the scientists' aim is fundamentally to gain as wide and deep a knowledge as possible of our physical surroundings. That this alone is a justifiable aim worthy of everyone's support is evident, for the first step toward control over our environment, which results in benefit to all, is a proper understanding of the phenomena and laws of the physical world. Our civilization, excepting our racial biological inheritance, is largely due to the activities of pure scientists. These scientists have not generally envisaged the modern applications or developments; this has been the work of engineers and chemists. But without the work of investigators in pure science the engineers would have no knowledge to apply.

That atomic research is the most profitable avenue of investigation of nature and its results the most useful tools for our control over our environment is very clearly brought out by looking briefly at the results that have been achieved. Our first quantitative knowledge of the atomic nature of matter, sketchy and imperfect as it was, can be attributed to Dalton at the beginning of the last century. The laws of the ordered proportions in which elements combine to form compounds pointed unequivocally to this hypothesis. It is from this advance more than any other that the modern science of chemistry is due. Dalton never dreamed of what the modern chemist can do with petroleum, coal tar, and cellulose but without the hypotheses associated with his name and those of his successors we would not have the fuel, power, the means of transportation, or the synthetic materials that make our lives more comfortable, painless, and effective than those of any other age. At this stage of development chemistry begins to branch away from the main trunk of natural philosophy which is physics. The ramifications of chemistry are too numerous for our further consideration and we shall confine our attention to those results of Dalton's theory that are more immediately related to the later discoveries of atomic research.

The developments of the past century were based largely on hypothesis of the discrete granular structure of matter, but with the beginning of this century we have entered on a more detailed understanding of the properties and characteristics of these atoms in different states of agglomeration and of the actual submicroscopic structure of these material units. The results of these pure scientific investigations have completely changed our material civilization within the past thirty or forty years. In our homes we have incandescent and glow-discharge lamps, and the latter are merely at the beginning of their development. We have electric heat and artificial sun light. We have communication with almost every place in the world by telephone, and the radio is an unparalleled disseminator of news and a source of information and entertainment. We have the combination of sound and moving pictures, and television which is still in its infancy. The power and communication industries would not exist as we know them without atomic research. The strength of large structures could not be tested without X-rays, and the electron microscope is just beginning to be applied to metallurgical and biological problems. In medicine atomic research has provided sources of radiation for local heating, X-ray photography and the treatment of disease by the entire spectrum of radiation from the infra-red through the ultraviolet to X-rays and radium. This has all occurred within the spans of our own lives and none of these things would be available had not the curiosity of such pure scientists as Becquerel, Thomson, Roentgen, Hallwachs, and Richardson been aroused to investigate seemingly trivial phenomena in their laboratories. This is the answer to, "What good is research in atomic physics?"

The research of the first quarter of this century may be considered to have laid the groundwork for our investigation of the nucleus or innermost core of the atom. This field is the newest and most fascinating and appears to have the greatest eventual possibilities of future benefit. While something was known of it before the last decade our first control over these phenomena dates from the work of Rutherford in 1919. Since study implies the possibility of controlling conditions nuclear research was born in 1919 and took its

first steps with the discovery of more powerful techniques by Rutherford's students at the beginning of the present decade. As research on the atomic nucleus is the result of the knowledge we have gained about the external structure of atoms, the methods of attack are fundamentally the same. These techniques themselves are of consuming interest for an enquiring mind because of the difficulty of the problem presented. We are investigating the completely unknown and must free ourselves as far as possible from all prejudices and preconceptions in order to make the most of discoveries as they are made and change our points of view, hypotheses and methods of investigation to evaluate properly the implications of our data. Atoms themselves are so small that we know we can never see them with our eyes and their cores or nuclei are as much smaller than atoms as they are themselves smaller than ordinary microscopic objects. None of our senses can ever detect single atoms but a scientist is not content without the most detailed information about them. And this information must be quantitative and highly precise. When so stated the problem appears hopeless. That it is not really so is partly due to the ingenuity displayed by the investigators, which represents one of the most remarkable intellectual triumphs imaginable, and partly to the common delusion that all our senses are able to yield us quantitative information and that the more of them we can bring to bear the more complete will be our understanding. Actually our only precise sense is vision and in the field of atomic physics it can only be applied indirectly. We must realize at the outset that we will never know how an atom looks, feels, or smells and that such properties as precise shape, sharply localized boundaries, smoothness, hardness, color simply have no significance as atomic attributes.

The question then arises: what can we know and how are we to go about finding it out? One answer is that large scale visual observations on the motion of microscopic particles which act as intermediaries between atoms and organisms on our scale provide a method of approach. Also in a few instances it is possible to observe visually the effects produced by very rapidly moving atoms. Our chief quantitative information, however, is received very indirectly but unam-

biguously through the refined techniques of electrical measurements and consists of a series of pointer readings. The information that we obtain is a rough estimate of atomic size, more accurate information about the forces that hold atoms together and that atoms exert on one another, and very precise information as to atomic masses or weights. The result is that we know atoms are of such a size that about a hundred million of them laid side by side would extend across your finger nail, the smallest weight that you can feel in your hand is a million million times greater than the force holding two atoms together, and about twenty-four zeros would have to be written before the figure representing the mass of a thimbleful of water before it would represent the mass of an atom. The determination of an atomic mass is representative of the complex indirect methods of measuring atomic quantities. In the first place, it must be known that an atom can be given an electric charge. Then we must know the magnitude of this charge. We must also have observed that a rapidly moving charged atom is deflected from a straight path into a curved one by a magnetic field. We must have a theory that tells us a relation between the velocity of the atom and an observable quantity such as a voltmeter reading, and one that relates the known charge, radius of curvature, magnetic field, and velocity with the unknown mass. Then from a voltmeter reading, a meter reading giving the magnitude of the magnetic field, and a length measurement that gives us the radius of curvature we are able to measure the atomic mass with an accuracy determined by the least accurate of all the measurements that have gone into a determination of this quantity. In this way we can find the masses of all the different kinds of atoms with an accuracy of about one per cent. However, relative masses, i.e., the ratio of the masses of two different types of atoms can be determined much more exactly as these relative measurements can be made to depend, for instance, on the ratio of the two lengths involved in measuring the radii of curvature and they can be found to one part in a hundredthousand. This general fact is true throughout all these types of measurements: absolute measurements in terms of length, mass, and time are difficult to make accurately, but in many instances

ratios are of the greatest importance and these can in general be found with much greater precision.

The next question of interest is what structure if any these minute entities possess. In posing this question we must realize that we can never answer it in the detail that we can, for instance, specify the structure of a watch or a locomotive. In the first place we learned through the work of Sir J. J. Thomson at the beginning of the century that electrons are constituents of all atoms. What is an electron? All we know about it is that there is a characteristic minute invariable electric charge associated with it and that it has an effective mass which is about a two-thousandth that of the hydrogen atom, the lightest one known. Furthermore it it acts as if it were a minute electrically charged spinning top for it possesses angular momentum and a magnetic moment. In addition to these facts, we know very roughly indeed the volume of space it occupies and that is absolutely all we know about it of this nature. As the electrons associated with an atom represent less than a two-thousandth of the mass of the composite system practically all of the atomic mass is associated with whatever is left after the electrons are removed. Also as the electrons have a negative sign by the convention generally adopted and the atom as a whole is neutral the massive part of the atom must carry a positive charge. Atoms of hydrogen, helium, lithium, and so on up to uranium are distinguished from one another by their properties which we find are almost entirely determined by the number of electrons they contain. This number is determined by finding how many of them we can remove in the simple cases and less directly for the heavier more complex atoms.

How are the electrons and the more massive constituent or constituents held together to form an atom? As these two components have opposite charges and we know opposite electrical charges attract one another, we have here one possible type of force. Our investigations have shown that there are other types of which we know very little. In the first place the identical nature of the electrons apparently gives rise to forces between them. In addition the spinning or vortical motion that appears to be associated with these elementary particles gives rise to spin forces of interaction in

somewhat the same manner as two smoke rings can be bounced from one another. The fact that I cannot describe these forces better shows that we have much to learn about them. But that they necessitate the performance of work to alter an atomic system is clearly established. The amount of work that must be done to produce various changes is something that is very accurately known through our study of the light that can be emitted or absorbed by various types of atoms. For the color, or wave-length, of the light emitted by an atom determines directly the energy change taking place. This type of spectroscopic investigation tells us many important things about the electron configuration of an atom from the energy point of view which is the most valuable type of information we can obtain, but nothing of course as to what the atom "looks" like.

We have at our disposal certain bullets or projectiles that travel so rapidly that the ordinary atoms composing matter are relatively at rest with respect to them. One source of these is the natural radioactive material found in nature the atoms of which may be described as minute fourth-of-July sparklers constantly sending out light of very short wave-length and also ejecting with high velocities material fragments resulting from their internal disintegration. These fragments have been investigated and found to be of two types. One type is the electron and the other was shown by Rutherford and Royds in 1909 to be the massive portion of the characteristic atoms of the familiar atmospheric gas helium. The electrons are light and easily deflected from their paths but the helium particles are about tenthousand times as massive and go rending through other atoms in their path leaving great havoc on an atomic scale behind them. A study of the way in which these projectiles are scattered by the atoms of other matter that they encounter was shown by Rutherford to imply very interesting consequences. The electrons associated with these scattering atoms are so light as to deflect the helium particles but little and the observed angular distribution of the scattered particles implies that the massive portions of the atoms responsible for the scattering are concentrated in very minute regions at the center of the electronic structure of an atom. The diameter of this central

mass or nucleus is of the order of a hundred-thousandth the diameter of the atom as a whole. The general relative scale and tenuousness of an atomic system is similar to that of our solar system where the central sun, though much smaller than the outermost planetary orbit, comprises most of the mass.

These facts and the additional one that radioactive emanations are given off in a statistically predictable way were all that was known of the matter up to 1919. No more was known because no way had been found of affecting the rate or manner of decay of natural radioactive material or of producing any change in ordinary stable atomic nuclei. Without being able to observe directly, or effect any observable alteration, no information could be obtained. The first step in effecting any change in an atomic nucleus was taken by Rutherford in 1919. In this year he observed certain phenomena that could be explained on the hypothesis that he had succeeded in transmuting a nucleus characteristic of nitrogen into one characteristic of oxygen. This simple but epoch-making experiment illustrates one of the techniques early employed in this type of work and also the type of evidence that is obtained and the method of reasoning involved. It is known that when a rapidly moving relatively massive particle such as an atomic nucleus impinges on certain crystals such as zinc sulphide, for instance, a tiny star of light appears. This is visible to a dark-adapted eye under suitable magnification. These tiny sparks are known as scintillations and indicate the stopping of the atomic projectile in the crystal. Their intensity is not a reliable guide to the exact type of atom stopped or to its original velocity. A crystal of zinc sulphide scintillates brilliantly in the presence of a bit of radioactive matter due to its continual bombardment by the helium particles that are emitted by this decaying matter. But if the separation between the radium and the sulphide is increased the scintillations stop quite abruptly at a critical distance due to the slowing down of the helium particles by collisions with the gas molecules in their path. When the gas in their path was nitrogen Rutherford found that a few fainter scintillations continued to appear long after the amount of intervening gas was sufficient to stop all the original helium particles emitted by the radium. The faint-

ness of the scintillations indicated that they were produced by a lighter projectile than a helium atom and the only lighter one known is that of hydrogen. Other confirming experiments showed that these were indeed cause of the faint scintillations and still further work showed that they came from the nitrogen and not from any hydrogen impurity. Rutherford then suggested the only plausible explanation which is that a type of nuclear reaction occurs when a helium nucleus impinges with a sufficient velocity on one of nitrogen and that one of the resulting products is a nucleus of hydrogen, which is generally called a proton. By the fundamental theorem of the conservation of electric charge the entity left is most likely an atom of oxygen for this has a charge equal to that of the sum of helium and nitrogen minus the single atomic charge carried away by hydrogen.

This was a discovery of the greatest importance which has led to the fascinating developments of nuclear physics in the past ten or fifteen years. The essential correctness of Rutherford's view was verified by Dr. Blackett, who used a beautiful technique due to C. T. R. Wilson. This is a method for rendering visible and photographable the path of one of these atomic projectiles through a gas. From the length of these thread-like tracks and from the apparent density of their image on a photographic plate, and their curvature in a magnetic field, the mass, charge, and velocity of the atomic projectile can be determined quite unambiguously. A collision with a nitrogen atom resulting in a disintegration is evidenced by the track of the helium particle from the radium suddenly forking; the long more tenuous tine represents the path of the hydrogen atom that emerges and the short heavy tine is that of the resultant oxygen atom that recoils in accordance with the law of conservation of momentum. Photographs of this type are probably the most informative evidence we have as to what actually takes place at an interaction between nuclei.

Though it was found that many of the lighter atoms could be disintegrated in this way, progress was relatively slow till the advent of new discoveries and new techniques six or eight years ago. In 1930 two of Rutherford's students, Cockcroft and Walton, showed that we do not have to depend

on the helium particles shot off by radium to produce these effects but that we can accelerate charged atoms of hydrogen and helium in electric fields until they gain sufficient speed to disintegrate other atoms that they encounter. These particles available in large quantities and with controlable speeds are the chief agents of modern research in this field. The fact that Rutherford's work showed that the law of conservation of momentum is applicable to this submicroscopic world enabled Chadwick, one of his colleagues, to prove in 1932 the existence of a hitherto unsuspected atomic particle with a mass closely the same as that of hydrogen but incapable of acquiring an electric charge. Hence this received the name "neutron." In the same year, at Pasadena, Anderson demonstrated the existence of a new light electronic particle having a characteristic positive charge, the opposite of that of the ordinary electron which had been known for thirty years or so. On the heels of these discoveries progress has been so rapid that I can only indicate briefly our present picture of atomic nuclei and some of the fascinating possibilities that are emerging as a result of these researches.

The fundamental and as yet indivisible particles that have been discovered by research and of which all atoms are probably composed in various proportions are the proton which is the hydrogen nucleus and the neutron. When recited baldly our knowledge of these particles does not seem extensive. We know their masses which are closely the same and their electrical characteristics, the proton having a characteristic positive charge and the neutron none at all. We also know very roughly the volume of space they occupy and that they have certain characteristic angular momenta, i.e., they behave somewhat like minute spinning tops. As the proton has a charge and is rotating we would expect it to behave like a small magnet which indeed it does, and somewhat to our surprise the uncharged neutron also appears to have magnetic properties. This is all we can say about these particles except that there are apparently forces between them that bind them in the characteristic complexes that we know as the nuclei of the atoms of all the different elements. These forces apparently have no analogues in our large scale world being of such a short range that they only become

evident on an atomic scale, but they are the object of present research and speculation by atomic physicists in many laboratories. When we know more about them which we can only do by further experimental work we will be able to understand much more clearly the many fascinating phenomena that we observe. At present all we know is that they do hold the fundamental particles together in stable groups and these stable configurations represent the nuclei of all the known elements. The simplest is the nucleus of hydrogen which is the proton itself. Then comes the nucleus of heavy hydrogen which is composed of one proton and one neutron. The combination of three of these particles appears for some strange reason to be relatively loosely held together or fragile for we do not observe it in nature while the helium nucleus which is formed of four, two protons and two neutrons, is a particularly strong and compact structure; so much so that it may even occur as a sub-unit in building other more complex atomic nuclei. The next lightest atom, that of lithium, is composed of three protons and either three or four neutrons and so on through the entire periodic table till we reach uranium, which is composed of 239 of these units, 92 being protons and the balance neutrons.

A study of the periodic table of the elements reveals many regularities and periodicities among the atomic nuclei. In the early portion of the table protons and neutrons occur in approximately equal numbers in the nuclei but as the atoms become heavier and more complex the laws of nuclear stability, of which we know very little, apparently require that there must be many more neutrons than protons in stable nuclei. We are just beginning to learn something about the forces that hold these complexes together and in our studies we have been able to make many new kinds of atoms which are not stable but throw off a light fragment or absorb one of their surrounding electrons thus forming again a stable configuration. The role of the ordinary electron and its positive counterpart is very imperfectly understood, for they appear to be emitted or absorbed by a nucleus though we have good reason to believe that an electron is quite unsuitable in nature to ever form part of a stable nucleus. Until we have learned more the mechanism of these processes must

remain pure speculation. As the proton and neutron differ characteristically by their charge the emission of a negative electron may be thought of as the change of a nuclear neutron into a proton accompanied by the simultaneous appearance and ejection of an electron. The emission of a positive electron is the converse process. Small quantities of mass also appear or disappear but with Einstein's relation between mass and energy we can relate this with the kinetic energy of the ejected particle or the very short wave-length electromagnetic radiation that frequently accompanies such a process. This relation ($E = mc^2$), that may be considered either as a consequence of the electromagnetic nature of mass or of the special theory of relativity, plays a very important role in nuclear theory. It states that there is a simple relation of proportionality between mass and energy. If the kinetic and radiation energies emerging from a nucleus are measured, the change in mass can be deduced. This affords a very accurate method of extending accurate mass measurements beyond the region of the lightest atoms into that of the heavier ones where direct comparison methods are relatively inaccurate. There is also the matter of the angular momentum associated with the particles that are ejected by a nucleus. This spin is apparently conserved just as we know it to be in large scale phenomena but it still presents many problems to the nuclear physicist.

As a specific example we may consider lithium which is one of the simplest and lightest elements we know, but one upon which a good deal of work has already been done. For the discussion of nuclear reactions the elements are generally laid out in a two-dimensional chart of which the abscissa is the number of protons or atomic number and the ordinate the number of neutrons in the nucleus. The sum of these coördinates is the total mass number on an arbitrary scale ($O = 16$). One type of lithium has three protons and three neutrons, this is the so-called isotope of mass 6, and the other has three protons and four neutrons, the isotope of mass 7. The bombarding particles at our disposal are neutrons, light and heavy hydrogen and the helium nuclei. We can also shine very short wave-length light which is called gamma radiation on the nucleus and that may disintegrate

it much as ordinary light liberates electrons from a metal plate in the photoelectric effect. When lithium of mass 6 is bombarded by high velocity protons so that the latter enter the nucleus the charge and mass number are increased by one unit apiece and beryllium of mass 7 results. This is not one of the known stable atomic species and we find that a helium nucleus splits off leaving a residue of mass 3 and charge 2, i.e., a helium-like atom. We do not know this as one of the ordinary atomic species either and it is likely that some subsequent event occurs to it resulting in heavy hydrogen though this process has not as yet been observed. One possibility is that the helium nucleus captures one of its two external electrons decreasing its net charge by one, i.e., becoming hydrogen of mass 3. As this is not found in nature either, we are led to assume that it is a very fragile structure which splits up into a neutron and hydrogen of mass 2 upon the slightest provocation.

If the lithium 6 is bombarded by helium nuclei the neutron and proton numbers each increase by two leading to boron of mass 10. The excess energy left with this nucleus, however, does not allow it to settle down stably but a neutron emerges which would produce an unknown species of boron of mass 9. This disintegrates by emission of a positive electron into the familiar stable berryllium isotope. If heavy hydrogen is driven at a high speed into a target of lithium a light and hitherto unknown beryllium of mass 8 would be formed. As we might expect this is not stable and a proton emerges leaving the well-known lithium atom of mass 7. It is apparently possible that this lithium atom is not formed in its ordinary configuration but is in a somewhat disorganized state from which it returns to normal with the emission of gamma radiation for this is observed and can only be attributed to a process of this sort. Finally, if neutrons are used as projectiles lithium of mass 7 results, but this is not the ordinary stable lithium of mass 7 but again a disorganized type of greater energy. We find that this can settle into a stable configuration in any one of three possible ways. One is by the emission of a gamma ray leaving lithium 7. As we find helium nuclei thrown off this represents a second mode of disintegration, one product of which is the heavy variety

of hydrogen of mass 3. As mentioned previously, this is not found in nature for it in all likelihood gives off a neutron at some later time and becomes ordinary heavy hydrogen. Finally, the lithium may, however, give off a proton resulting in another unknown element, helium of mass 6. This exhibits a new phenomenon for it disintegrates slowly as radioactive atoms do, a neutron changing into a proton, which is evidenced by the external appearance of an electron. The mean life of this type of helium is of the order of a fraction of a second before this shift occurs and lithium of mass 6 again results.

A study of other types of atoms when subjected to bombardment by these high velocity projectiles yields further information of this general type but with the appearance in certain instances of positive electrons and other less well understood phenomena. Also if our ideas are correct the incidence of gamma rays on lithium of mass 7 should result in the emission of a neutron and the production of lithium of mass 6. As yet a suitable source of gamma rays is not available to test this hypothesis. This simple discussion of nuclear phenomena by means of our diagram resembles to some extent a type of chess. One has certain squares representing stable elements from which we must begin and on which we must probably end. The possible initial moves are determined by the type of projectile for if electric charge and large units of mass are conserved the end of the first move has coördinates determined by the sum of all the protons and neutrons involved. The second move depends on the element which is formed and the state of disorganization in which it is left. We have observed helium nuclei, protons, and neutrons to be emitted in various instances and also both positive and negative electrons, probably evidencing a proton-neutron or neutron-proton conversion within. In some instances we have observed two successive processes but in these one or two moves the process completes itself and we are back on one of our permitted squares representing a known stable element. From observations on the energy put in or liberated in its various forms of motion, mass, and radiation we have learned something more than I have been able to tell you of nuclear mechanics. But it is still essentially a great unexplored field which is rich in scientific possibilities.

We have seen that research in atomic physics has amply justified itself to society by its important applications which have almost remade our technical civilization. We might ask whether the field of nuclear physics has any such potentialities. The answer is undoubtedly in the affirmative though it would indeed be a rash prophet who would undertake to state exactly what developments may be expected. It is impossible to tell how soon our knowledge of, and control over, these nuclear processes will have any reaction on our everyday life, but the processes of technical development and dissemination are apt to be even more rapid than in the case of atomic physics. We can only consider here one or two of the possibilities that are at present being tentatively explored. In the first place the possibility of obtaining power from the nucleus is not entirely visionary. With our present techniques the process is very inefficient though great strides have already been made in this direction. Furthermore, this power is of a highly specialized kind and will require as yet undeveloped engines for its utilization. In the second place the neutron which is a little-known entity may be either directly or indirectly of biological significance. Its direct effect on tissue and its various constituents will certainly differ from that of X-radiation for instance, but it is too soon to say just what its effects will be and in what instances they will prove beneficial.

The possibility of making a small fraction of the atoms of almost any substance radioactive is a development with unbounded potentialities and many interesting applications have already been made. The direct therapeutic effects of the radioactive variants of ordinary elements are the same as those of radium and if there were no other result than the production of large amounts of equivalent radium it would be one of the most important discoveries ever made. The fact that atoms can be endowed with this radioactive property without losing their ordinary chemical properties makes the work of much greater importance than the discovery of any amount of radium would be. As a single instance the thyroid gland is known to have the property of assimilating iodine. It is possible to render iodine radioactive in such a way that it decays with a mean life of about 25 minutes with the

emission of ordinary electrons. Thus radioactive iodine injected into the body will tend to be localized in the thyroid gland and affect it specifically in a way not possible with ordinary radium. Also the atom resulting from the emission of the electron is no longer radioactive and peculiarly inert chemically which removes any possible hazard of the treatment. In a similar way phosphorus and calcium which are bone constituents may be rendered radioactive localizing radioactive material in the bony structure.

The applications in the closely associated field of physiology are of equally great interest. As it is easy to detect the presence of a very minute number of radioactive atoms among a great number of the ordinary variety characteristic of the element it is possible to prepare samples of elements that retain their identity and may be followed through physiological reactions. It has been reported, though not as yet verified, that long-lived radioactive carbon atoms can be prepared. These may form molecules of organic compounds which can then be followed through the body opening up a new and highly important field of biological research. The physiologist can investigate the details of processes for which no technique was previously available and hitherto unsuspected phenomena may be uncovered. As an instance it is known that the thyroid assimilates iodine but if other glands assimilate selectively specific elements they do it in such small quantities as to be undetectable at present. However, if the elements were introduced in a radioactive form simple and powerful techniques of detection could be employed which would bring any selective assimilation to light.

I can only refer even more briefly to the application of this technique in other fields. Radioactive "tracer atoms" can be employed in chemistry to follow chemical elements through exchange reactions. They may also be used to investigate the baffling problems of catalyst poisoning and surface adsorption. In metallurgy and the physics of solids they may be used to study the diffusion of atoms through single crystals and across and along crystal boundaries. Studies of this sort under various conditions of temperature and strain should clarify many of the problems that are now presented by the solid state and lead to alloys with more

desirable properties for all the various applications. Applications to many other fields of science and also to industry suggest themselves immediately, though our time is too short to consider them. If nothing emerged from nuclear studies but this one technique I doubt if the present equipment in all the laboratories in this country would be adequate to supply the radioactive material for the applications that will develop within the next few years. We are undertaking this type of work at the University of Pennsylvania to the extent that our rather limited funds permit and we are convinced that this vast unknown field has even more important possibilities in store for us. We hope that we shall be able to interest sufficient support not only to contribute this technique to physics and our sister sciences, but to go on and explore the very promising field of the nucleus with every confidence that the understanding gained thereby will lead to still more important developments in the future.

Sterilization by Ultraviolet Radiation.—S. G. HIBBEN AND P. W. BLACKBURN. (*Electrical Engineering*, Vol. 57, No. 11.) A new use for electricity rapidly is developing in the field of sterilization through controlled applications of radiation in limited portions of the ultraviolet spectrum, that in the neighborhood of 2900 to 200 Angstroms. Such radiation has been known sketchily for its abiotic properties, but a practical knowledge leading to commercial applications has evolved only recently from careful segregation of different wave-lengths, the elimination of certain very short-wave radiations that seem undesirable, and the perfection of a means of measuring such radiation quantitatively. The new tool is in the form of a tubular lamp, known as the Sterilamp. It is an evacuated tube into the ends of which are sealed special metallic electrodes coated with electron-emitting material. Into the tube is inserted a small quantity of inert gas which permits ionization at room temperatures, allowing an arc discharge to become established immediately when electric power is applied. The tube also contains mercury vapor at a pressure which results in the emission of negligible radiant energy in the visible spectrum but produces ultraviolet mercury lines. Sterilizing installations, because of recently perfected methods of measurement of abiotic radiation, will in the future be planned with an exactness equalling that of the normal lighting installation. Naturally, numerous circumstances will influence results but on a comparative basis if 1000 units of abiotic radiation are required to kill black mold then to kill other organisms the ratios are *N. catarrhalis* 60–85, *B. subtilis* 25–30, *E. coli* 15–20, *E. typhosa* 7–10. Many of the applications of the sterilizing properties of ultraviolet radiation have been investigated. They include the common bacteria found on rims of drinking glasses, the preservation of meat, and air sterilization. Many more are constantly arising.

R. H. O.

ON HIGH SPEED WIND TUNNELS.

BY

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SUMMARY.

The experiments on efflux of gases from orifices show that important changes occur in the properties of a gas accelerated to high velocity, apparently due to changes in the distribution of molecular energies. These changes indicate that conditions of the air flow around a body depend not only on the relative velocity, but on the absolute velocity of the air as well. Therefore, the results of the model tests obtained in wind tunnels at the velocity approaching that of sound do not correspond to the practical case of a body moving through still air. In particular, the occurrence of the compression shock appears to represent the characteristic of the high speed air stream, rather than that of the high relative velocity of the model and the air.

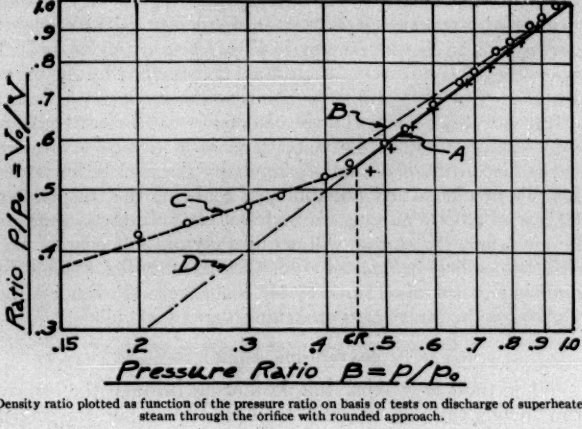
The method of testing models in wind tunnels is based on the principle that flow of the air and the aerodynamic forces produced by it depend only on the relative velocity of the air and of the body, and that exactly the same results are obtained by either moving the body through still air, or making the air to flow past the stationary body. While this principle appears to be self evident, it involves the assumption that physical properties of the air are not affected by its own velocity. This assumption would have been correct if the molecules of air were at rest, or were moving in the direction and with the velocity of streaming. This, however, is not the case. The molecules of a gas are in constant random motion of translation and rotation, which are superimposed on the streaming motion. As the total energy possessed by a quantity of gas in accelerated flow remains constant, it follows that velocity of random motion of its molecules must be decreasing in order to provide for the increasing energy of streaming. Moreover only the effect of translational velocities of random motion of molecules is directly observed as the pressure of the gas, while the energy of rotation represents a reserve from which the added energy is transmitted to trans-

lational motion of molecules, governing the rate of change of density ratio with pressure ratio during expansion.

The variation of gas density with pressure, and the nature of exchange of molecular energies are not important in case of low air speeds, at which the air can be considered as incompressible, and the technique of wind tunnel testing at low speed is entirely justified. This technique was extended to the high speeds approaching and exceeding the velocity of sound without proper research in the resultant changes of the properties of the air. The direct experimental data on the air are therefore not available, but important conclusions can be derived from the data on superheated steam obtained in connection with research on steam turbine nozzles.[1] It will be shown later that circumstantial evidence obtained in aerodynamic experiments supports the conclusions based on the superheated steam data.

In the high speed type of the wind tunnel [2, 3] the air originally at rest in the laboratory at pressure p_0 is accelerated through the inlet flare to the velocity u_1 at the pressure p_1 in the test section, and is further accelerated to the velocity u_2 at pressure p_2 in flowing around the model under test. The model is located very near the entrance flare, usually within the distance of one to two times the diameter of the test section, and there is but a very short interval between the acceleration in the inlet flare and the acceleration in the flow around the model. As far as the changes of pressure and density are concerned this flow can be considered as identical with the flow through the orifice with rounded approach, which was studied by the author in two previous articles.[5, 6] Figure 1 taken from the previous article shows the relation between the density ratio and the pressure ratio of the superheated steam flowing through the orifice. The salient feature of this plot is the well defined break in the curve at the pressure ratio of 0.45, corresponding to the steam velocity somewhat exceeding the velocity of sound. At this pressure ratio the expansion of the gas ceases to follow the expansion law for the polyatomic gas, and continues the expansion following the law of the monatomic one, indicating that transfer of energy has ceased between the rotational and translational velocities of molecules. In this work the author will refer to the point

FIG. 1.



Density ratio plotted as function of the pressure ratio on basis of tests on discharge of superheated steam through the orifice with rounded approach.

at which this change occurs as the "critical pressure ratio" and as "velocity of sound," merely because these are the familiar expressions. The exact definition of this point is uncertain. Figure 1 shows the break in the curve at the pressure ratio of 0.45; the critical pressure ratio is defined in steam engineering as the point of maximum discharge by adiabatic formula, corresponding to the pressure ratio of 0.54, and the velocity of sound is usually taken in aerodynamics as defining the change of the air flow.

As long as the maximum speed u_2 of the air affected by the presence of the model in wind tunnel corresponds to the pressure ratio above critical, the whole range of the pressure ratios involved is contained within the curve A of Fig. 1. In such a case the results obtained in wind tunnel can be used to predict the aerodynamic forces acting on the same model moving through the still air. The conditions, however, are not identical, and in particular the viscosity, which is governed by translational velocities of molecules, is not correctly represented, but the functions are continuous, and the proper methods of correcting test results may be developed.

The conditions are entirely different in case of the test conducted at the air speed u_1 corresponding to the pressure ratio p_1/p_0 just *above* critical, so that velocity at the model u_2 corresponds to the pressure ratio p_2/p_0 *below* critical. In such a case the air condition in the tunnel is described by the curve A of Fig. 1, while the air condition at the model is described by the curve C. The change of density ratio with pressure ratio with increase of the velocity from u_1 to u_2 is expressed by the discontinuous function shown by the broken line A–C, and this precludes any possibility of reducing the test data to the case of a body moving through still air. In the latter case the air originally at rest will acquire certain velocity $-ku_1$, with the coefficient k usually much less than unity, so that air condition is described entirely by the curve A, even for the velocity of the body u_1 approaching that of sound.

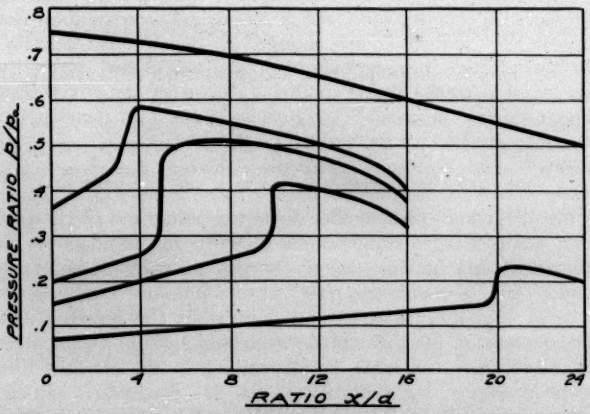
COMPRESSION SHOCK.

While the gas is expanding in flowing through the orifice, or in flowing through the entrance flare and around the model in the test section of a high speed wind tunnel, the velocity and the energy of translation of random motion of molecules diminishes as the energy of streaming motion increases. The interchange of energy in incessant collisions of molecules maintains the equipartition of energy between translational and rotational energies of molecules, so that rotational energy is also made available for the transfer into streaming velocity. This transfer of both rotational and translational energies of molecules into the energy of streaming determines the slope of the curve A of Fig. 1, i.e. determines the relation between the density ratio and the pressure ratio during expansion. At certain (critical) value of the pressure ratio in accelerating flow of a gas the transfer of energy ceases between rotational and translational motions of molecules, and the gas continues to expand as monatomic one, following the curve C. The important characteristic of a gas in this state is the fact that energy contained in the rotational motions of molecules remains constant at the level corresponding to the critical pressure ratio, while the energy of translation continues to diminish with increasing streaming velocity. The excess of rotational energy of molecules, temporarily unavailable in the

energy transfer, is represented by the horizontal distance between the actual expansion curve C and the extension D of the curve A for any given pressure ratio below critical.

The suspension of the energy transfer between rotational and translational velocities of molecules depends in the unknown way on the condition of accelerating flow, and the energy transfer is restored shortly after cessation of acceleration. This brings about a more or less sudden transfer of the excess of rotational energy into translational one, and is manifested by a sudden rise of pressure of the gas. This phenomenon is well shown by the tests of W. Frössel [7] on the flow of air through pipes, and by comparison of his tests with tests on the discharge of superheated steam from the orifice.[1] Frössel measured the pressure ratio in the pipe with rounded inlet, which may be visualized as an extension tube attached to the orifice with rounded approach. Figure 2

Fig. 2.



Variation of pressure of the air flowing through the pipe with the distance from the inlet for the pressure ratios at the inlet above and below the critical one.

shows some of the curves obtained by Frössel. The pressure ratios p/p_0 are plotted as ordinates, and the ratios x/d as abscissæ, x being the distance from the inlet to the pressure

measuring hole, and d is the diameter of the pipe. For the pressure ratio at the inlet above critical there is observed a continuous *fall* of pressure corresponding to the loss of energy of the stream in overcoming frictional resistance of the pipe. For the pressure ratio at inlet below critical there is observed a steady *rise* of pressure for a short length, then a *sudden increase* of pressure, followed by the gradual *fall*. It appears therefore that for the short distance after the inlet into the pipe there is gradual transfer of the rotational energy of molecules into translational one, sufficient not only to balance the frictional loss, but to cause the increase of pressure despite this loss. The gradual transfer of the part of this energy is followed by the spontaneous equalization of it, causing the sudden rise of pressure. Upon completion of these phenomena, representing the change in the state of air from that shown by the curve C to that shown by the curve D of Fig. 1, the gradual fall of pressure due to frictional losses reappears. It must be emphasized that sudden rise in pressure in compression shock described above does not necessarily indicate any change in density or velocity. In the change from metastable condition of air shown by the curve C to the stable condition shown by the curve D of Fig. 1 the rise in pressure may occur at constant density, i.e. at constant velocity for a given mass flow.

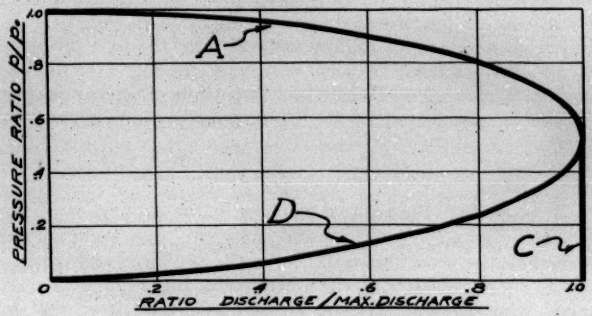
DISCHARGE OF GAS THROUGH THE PIPE AND THE ORIFICE.

Still further proof of the above reasoning and of the fact that such changes occur in the flowing gas is afforded by the study of the amount of the gas discharged from the orifice [1] and through the pipe [7] at the pressure ratios below critical. As long as the relation between the pressure and the density of the gas can be expressed by the exponential relation $p/(\rho)^k = \text{Const.}$, corresponding to the straight line A–D of Fig. 1, the quantity of the gas discharged can be expressed by the well known adiabatic formula. The interesting feature of this formula is the fact that it shows the maximum for the amount of discharged gas at the pressure ratio:

$$p/p_0 = [2/(k+1)]^{k/(k-1)}.$$

The quantity of the gas discharged increases up to the maximum at this pressure ratio, and diminishes with further reduction of the pressure ratio, as shown by the curves *A* and *D* of Fig. 3, marked with the same letters as corresponding

FIG. 3.



Discharge of a gas through the orifice (curves *A* and *C*) and through the pipe (curves *A* and *D*).

curves are marked on Fig. 1. This curve has been found to conform to actual facts in case of the discharge of the air through the pipe in experiments of Frössel.[8] On the other hand the adiabatic discharge formula is well known to fall down in case of the discharge of the gas from the orifice in which case the discharge does not diminish for the pressure ratios below critical, but remains constant as shown by the line *C* of Fig. 3, corresponding to the line *C* of Fig. 1. The numerical evaluation of the discharge from the orifice for the pressure ratios below critical was given by the author in the previous article.[6] In this work this fact is of interest only in the sense that it helps to formulate the clear picture of the changes in the properties of the gas. The reason for the breakdown of the adiabatic discharge formula for the pressure ratios below critical, and the fact that discharge remains constant is adequately explained by the change in the slope between the curves *A* and *C* of Fig. 1. For any pressure ratio below critical the density of the gas shown by the curve *C* is greater than that shown by the curve *D*, and therefore greater quantity of the gas can be discharged at a

given velocity. The fact that in Frössel's experiments on the flow through the pipe the discharge follows the one indicated by the adiabatic formula at the pressure ratio below critical represents the proof that state of the air at the exit from the pipe corresponds to the curve D of Fig. 1. On the other hand the state of the air at the entrance to the pipe is the same as at the throat of the orifice, which corresponds to the curve C. Between the entrance to, and the exit from the pipe the state of air changes from the one shown by the curve C to that shown by the curve D, and the liberation of energy in this change is manifested in the changes of pressure shown on Fig. 2.

INTERPRETATION OF WIND TUNNEL DATA.

It is evident that above changes in the properties of the air, as it is accelerated to the velocities approaching and exceeding the sound velocity, have the profound significance in the interpretation of the phenomena observed in a high speed wind tunnel. As the air is accelerated from rest in flowing through the inlet flare of the wind tunnel, approaches the model with velocity u_1, and is further accelerated to the velocity u_2 in flowing around the model, the state of air changes from that shown by a certain point on the curve A of Fig. 1 to that corresponding to a point on the curve C. As the acceleration of the air ceases after passing the maximum section of the body, and deceleration begins, the transition from the metastable state C to the stable state D begins, with the occurrence of the compression shock, as the conspicuous feature of this transition.

It is emphasized that the occurrence of such a compression shock is characteristic for the method of the wind tunnel test, and not for the relative velocity of the model and of the air. In case of the body moving in still air, the air is displaced, and is caused to flow past the model with the moderate velocity numerically equal to $u_2 - u_1$. Even if the speed of the body approaches that of the velocity of sound, the air originally at rest is accelerated to much smaller velocity. Therefore the critical pressure ratio in accelerated air is not reached, and one would not expect the occurrence of the compression shock, such as is observed in high speed wind tunnel.

A relatively small mass of air on the streamlines originally near the centerline of the body is accelerated to the speed of the body (at stagnation point) with corresponding rise in pressure, and thereafter expands and accelerates in flowing past the body to the same pressure ratio as in wind tunnel, but as this mass of air is small in comparison with the total mass of air flowing past the body, the resultant compression shock, if it occurs, must be very feeble. The conditional "if" in the above sentence has to be inserted because the probable behavior of this small mass of air cannot be predicted in view of the lack of experimental data. The air at stagnation point is accelerated to the velocity of moving body by its impact, i.e. the additional energy is imparted to it. Further experiments on the impact of the jet, equivalent but directly opposite to the experiments on the efflux from the orifice, must be provided before the behavior of air in such a case can be predicted.

EXPERIMENTAL DATA ON COMPRESSION SHOCK.

The above reasoning indicates that compression shock occurs in the high speed wind tunnel, but cannot be expected to occur in case of a body moving through the still air at the same relative velocity. The experimental verification of this can be found in the application of the aerofoil test data obtained in the high speed wind tunnel to the calculation of the propeller performance, and in comparison of these calculations with the propeller test data.[4] For the tip speed of the propeller equal to 0.75 of the speed of sound the computed and the observed characteristics check very well, both indicating the same efficiency of the propeller of about 72 per cent. For the tip speed of 0.90 of the velocity of sound the measured characteristics of the propeller remain practically unchanged, while the computations based on the high speed wind tunnel test of aerofoils show the drop of efficiency to 56 per cent. This means that adverse phenomena (compression shock) which cause the loss of aerofoil efficiency at this speed in wind tunnel do not occur in case of the propeller blades moving through the air of low absolute velocity.

The comparison of the photographs [9] of the shock wave taken in N.A.C.A. High Speed Wind Tunnel in test of the

aerofoil, and of the bullet moving through still air indicates entirely different nature of the air condition in these two cases. The photograph of the aerofoil taken in the high speed wind tunnel shows that:

a. Large volume of the air is affected by the shock wave.
b. The disturbances just begin at the maximum cambers of the upper and of the lower surfaces.
c. The disturbances increase gradually over a short length of travel, and then suddenly acquire large proportions.

All of the above characteristics check well with the sequence found in the tests on the flow of air in pipes by Frössel. In particular large proportions of the disturbance is emphasized, as this is to be expected from the spontaneous change of the state of the air.

On the other hand the photograph of the bullet moving through the still air shows isolated threadlike images indicating well localized wave formations, the most pronounced of which is forward of the nose, and evidently represents the sound wave caused by the high compression of the small mass of air at stagnation point. This photograph of the bullet shows the wave formations closely resembling the waves formed by a vessel moving on the surface of water. It was shown by Riabuchinsky [10] that such waves formed on the surface of water may represent the conditions of the flow of compressible air. The photograph of the bullet can be interpreted in this light as showing the wave formation in the compressible fluid, but not the sudden changes of the state of air as are found in the compression shock in a pipe and in the wind tunnel.

REFERENCES.

1. G. B. WARREN AND J. H. KEENAN. A Machine for Testing Steam Turbine Nozzles by the Reaction Method. The American Society of Mechanical Engineers, 29 West 39th Street, New York, N. Y., Reprint No. 2001 of the paper presented at the Midwest Power Conference, Chicago, Ill., January 26 to 29, 1926, Table I on page 42 and Fig. 15 on page 56.
2. A. BAILEY AND S. A. WOOD. The Development of a High Speed Induced Wind Tunnel of Rectangular Cross Section. A. R. C. R. & M. No. 1791, Great Britain.
3. JOHN STACK. The N. A. C. A. High Speed Wind Tunnel and Tests of Six Propeller Sections. N. A. C. A. Technical Report No. 463.

4. JOHN STACK, loc. cit. supra, Figs. 22 and 23.
5. B. V. KORVIN-KROUKOVSKY. Efflux of Gases Through Orifices—Kinetic Theory. JOURNAL OF THE FRANKLIN INSTITUTE, **225**, No. 1, January, 1938, pp. 81–93.
6. B. V. KORVIN-KROUKOVSKY. The Flow of Compressible Gas. JOURNAL OF THE FRANKLIN INSTITUTE, **227**, No. 1, January, 1939, pp. 99–110.
7. W. FRÖSSEL. Flow in Smooth Straight Pipes at Velocities Above and Below Sound Velocity. N. A. C. A. Tech. Memo. No. 844.
8. W. FRÖSSEL, loc. cit. supra, Fig. 8.
9. C. N. H. LOCK. Problems of High Speed Flight as Affected by Compressibility. *The Journal of the Royal Aeronautical Society*, Vol. XLII, No. 327, March, 1938.
10. DIMITRI P. RIABOUCHINSKY. Thirty Years of Theoretical and Experimental Research in Fluid Mechanics. *The Journal of the Royal Aeronautical Society*, Vol. XXXIX, Nos. 292 and 293, April and May 1935, p. 439.

New Chemical Fiber Surpasses Silk.—(*Chemical and Metallurgical Engineering*, Vol. 45, No. 10.) A new synthetic fiber whose superior elasticity, strength and water resistance may enable it to displace further silk and several other natural products of commerce has been developed by E. I. duPont de Nemours & Co., Inc. Tests described in U. S. Patents 2,130,947 and 2,130,948 show that the new fiber known as Fiber 66 has properties that may well enable it to penetrate the silk hosiery field, the last remaining stronghold of natural silk. It is a thermoplastic polyamide resin formed by heating in substantially equimolecular amounts a diamine and a dibasic acid or an amide-forming derivative of a dibasic acid under conditions of condensation and polymerization until the product has a sufficiently high molecular weight to show fiber-forming properties. Production costs for the finished polyamides are high at the present stage of development, but their eventual market price is not yet determinable. The polyamides can be spun into continuous filaments directly from the reaction vessel, or they can be dissolved in a suitable solvent and then extruded into a liquid which dissolves the solvent but not the polyamide, or the solution can be extruded into a heated chamber where the solvent evaporates. By similar processes, bristles, sheets, ribbons, rods and tubes can also be formed. A significant characteristic of the spun filaments is their ability to increase greatly in strength and pliability upon cold drawing. It is claimed that cold drawing, which may be as much as 200 to 700 per cent. without breaking the fiber, leaves the filament permanently extended, much stronger and more elastic. Also, examination by X-ray shows true fiber orientation in the drawn filaments but not in the undrawn filaments. The cold drawing referred to here differs from stretch spinning known to the artificial fiber art in that it may be carried out in the absence of any solvent or plasticizer.

R. H. O.

WHAT HAS BECOME OF REALITY IN MODERN PHYSICS?*

BY

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In starting to prepare an address having to do with reality it seems not inappropriate to go to the dictionary and see what it has to say about the word, so I went to the dictionary. I will not tell you all that it said. It seemed to be having a hard time trying to say anything; and, indeed, I came to wonder whether the dictionary would permit me to say that the moon is real. I became more convinced of the truth of a statement I have had occasion to make several times during the past few years, and to the effect that "Words are merely imitative grunts invented by mankind for the purpose of putting minds into harmony with one another." They are the catalysts of thought; but it always remains for the parties concerned to become adjusted in their thinking, so that each gains a correct comprehension of what the other wishes to convey.

Any fairly general concept such as that of reality is apt to have about it, in addition to the elements which really matter, other elements of an irrelevant nature. These irrelevant garnishings frequently absorb most of the limelight; and being frail tinselled things of the type which catch the eye of the mind most readily, they are the things most vulnerable to the attack of anyone who sets out to question the logical meaning of the concept and the basic foundations beneath it. Even as the eye of vision sees more readily the surface of things than the structure within, and seeing that surface sees frequently for the most part the dust of other things which has fallen upon it rather than its true self, so the eye of the mind, in looking at the concepts of nature, sees first the surface rather than the inner structure

* An address presented before the Carnegie Institute of Technology, Chapter of Sigma Xi, in Pittsburgh, Pa., on April 2, 1938, and also at the State University of Iowa, on November 21, 1938.

which binds it together, and in that surface itself sees many things which have fallen upon it and become entangled with it, but are no part of its fundamental nature. Sometimes it is difficult to find the real working element which forms the essential part of a concept on account of its complete entanglement with the irrelevant parts which obscure it.

There are three states of sophistication in physics, and particularly in modern physics. In the first, one accepts, in a superficial way, everything at face value. In the second, he sees that many of the verbal statements require qualification. Some of the concepts are not unambiguous in meaning. Certain conclusions are limited in their validity. The significance of the statements is not quite what is implied by the words. The arguments presented are not a proof of something, but an analytic expression of a hypothesis, and so forth. In this state of sophistication one frequently thinks by analogy in the belief that concepts with which he is familiar in one line of knowledge can be transported without change to another. State of sophistication No. 2 is destructive rather than constructive. There is a tendency to look, not for the rose, but for the thorns. Those in state No. 1 are the natural prey of those in state No. 2. The latter can quite readily knock all the arguments of the former into a cocked hat, and can show that everything they say is illogical, meaningless, and contradictory. Unfortunately, those in the realm No. 2 are apt to vision the complete defeat of those in realm No. 1 as the ultimate goal to be attained. In state of sophistication No. 3, one admits many of the points raised in state No. 2, but realizes that the main essence of the theories under discussion lies in realms other than those in which one in state No. 2 is looking for them. A member of state No. 2 says to a member of No. 1: "You are a fool, and a waster of the time of yourself and everybody else. Your arguments are illogical and your theories are all nonsense." One in realm No. 3 is more sympathetic. He says to the disconsolate resident of state No. 1: "My friend, your language is terribly muddled. You do not propose to yourself quite the right questions, nor do you give quite the right answers to the questions you do propose. Now in connection with this matter, for instance, what you really should have said is

such-and-such." No. 3 admits that there was something to be said in that vicinity, and he criticizes only the method of saying it. In state No. 3 one looks in the muddled structure of state No. 1 for the real thought structure which binds the facts together as with bars of solid steel. He sees that the denizens of No. 1 have painted the steel bars with gold paint to call attention to them, that they have inadvertently rotted them through in certain places and spilt some of the paint in all sorts of other places all over the structure. In state No. 2, one complains of the gold and says that it would not serve to hold the structure together. In state No. 3, one sees that the gold paint is really irrelevant.

If I raise with you the question of whether I am in reality here this evening, you will probably believe that there is no question about the matter. You will assert that you see me here, a three dimensional object, with length, breadth, and thickness, emitting those imitative grunts to which I have referred and which you are conscious of hearing. What can constitute more satisfying evidence as to my presence? Alas, I have to inform you that, without going further into philosophy than is involved in the elementary principles of high school physics, you see me twice over, once in each eye, you see me as two-dimensional images upon the retinas of your eyes, you see me upside down, and that which the left eye sees the right hand side of the brain interprets. My presence here is really a very doubtful and complicated affair, and I am so perturbed as to the reality of my own existence that I am tempted to seek the confidence of words, if you ask where and what I am, by exclaiming with Popeye the Sailor, "I Yam what I Yam, and what I Yam I Yam."

If I were a mythological or a theological being, you could thump the table in righteous indignation at any suggestion to the effect that I do not exist and bring the fear of eternal punishment to the hearts of all who would for a moment permit such a heresy to occupy their thoughts. Your minds would be closed to the search, and you would be saved needless worry. Since, however, I am but a humble man of science, you cannot so easily escape the necessity of saying what you mean by my presence, and you will not be subjected to such pains and penalties by questioning my existence.

Now, in spite of all the difficulties I have raised as to the reality of my presence, the fact is that you seem to have had no doubt about it, otherwise you would not be here—if, indeed, you are here. In state of sophistication No. 1, I should never have had any trouble about the matter. In state of sophistication No. 2, I am highly elated at the possible deception I have perpetrated upon you in making you believe that I am here, and in the deception I have perpetrated upon myself in becoming conscious of the fact that I am talking to you when you may not be here at all. In state No. 2, I cry aloud "The gods be praised, we are all mad!" In state No. 3, I pin myself solidly to the faith that there is really something to be said about the matter, even though we are all under an illusion as to what it is and as to what is going on. I will at least seek elements in terms of which to correlate the illusions, if I find them; then, in state No. 3, these elements shall, indeed, be the things which are real. A denizen of state No. 1 will probably be much bewildered by them. For his comfort, I shall probably try to piece them together in some kind of way which will recreate things which will correspond to and be representative of his somewhat naive thoughts. A denizen of state No. 2 will probably call me a metaphysician; or if he gets to the point of understanding what I am talking about, he will look for fresh trouble there. But in state No. 3, I shall occasionally do things for which one in state No. 2 will scorn me. I shall think sometimes even as the simple fellow in state No. 1 thinks. My extra strength will lie in the continual caution which, while permitting me to stimulate my brains with the naive thoughts of state No. 1, will keep those thoughts as my servants rather than as my masters, and will enable me to know when one of them is liable to lead me astray.

If you think it is my purpose this evening to delve into the morasses of scientific theories and philosophic thought and bring forth, cleaned up for your inspection, something of which I can say: "Here is reality, it was covered with dirt and mildew, so that you could not see it. Now that it is cleaned and free from grime, you recognize it as something which you have long known and for which you were looking." —If you believe this, you are doomed to disappointment.

No, my mission must be other than this. I am as one who, in the midst of his labors, is called upon by a professor of English, who says: "My dear friend, I have here a word, 'reality.' It is a very old word and I hate to throw it away. Can you find in your science something which you can christen with this word? It is a word which has been used much, but I have heard you say that its uses have been inconsistently made, and you have threatened to take away its job and have it removed from the dictionary. I ask you not to give it back its old job, for you tell me it really had no job at all. I beg of you, however, for the sake of history and for the dignity of the dictionary, to provide something for it to do." I am fond of my friend, the professor of English. His subject has been responsible for many beautiful things in life, and I shall try to accommodate him.

It may be worth while to pause for a moment and, without criticism of his reasons or motives, enquire what criteria man has in the past been accustomed to use as a guarantee for the reality of things. In spite of the remarks I have already made throwing doubts upon your justification for believing that I am here this evening, I suppose the most common evidence which man has accepted in the past as to the reality of anything is the evidence of his eyes. If he can see it, it is real. Truly, he has had to qualify this attitude occasionally, for even a cat soon finds that it is useless to try and fight what it sees in the mirror. If you raise the question of the mirror to one who was content with what he saw, he would probably have to retrench a little to the extent of saying that if a thing could be seen, the fact constituted evidence of its existence, but not necessarily of the position that it occupied. Then I suppose the sense of touch has provided another criterion. If you can feel a thing, it must be there. Here again I fear a few reservations are necessary, for I have heard tales of a man suffering pains in a big toe which has been amputated several years previously. One who relied upon the sense of touch would have to retrench a little more than he who relied upon the sense of sight; for in the instance cited, the best he could say would be that his sensations were evidence that there was a big toe which had been real. The remaining three senses, smell, taste, and

hearing, have all, in varying degree, and with their appropriate reservations, served to provide man with criteria for what he calls the reality of things.

When we pass from the direct evidence of our senses to the next realm of criteria, we come to such evidence as that which I have for the existence of New Zealand, which I have never seen, touched, smelt, tasted, or heard, for I have never been there. Somebody once told me there was such a place. He may have said that to deceive me, but such a lot of other things connected with the assumed place and with people who are said to have come from there have come to my notice, and I can correlate all of these things by supposing New Zealand to exist. This is an example of adoption of a criterion of reality founded upon the fact that the assumption of the existence of the thing in question harmonizes a number of phenomena. It is quite true that in the case of New Zealand the said harmonization was enhanced as a criterion of reality of that land by the fact that a lot of my friends have seen it even though I have not. We can, however, cite instances of similar situations in which harmonization of events has been taken for the criterion for reality of something which nobody had ever seen, or at any rate, which nobody had seen at the time when the criterion was accepted. One of the most famous illustrations of this kind is that associated with the discovery of the planet Neptune. The heavenly bodies were found to move in accordance with certain elegant laws, prominent among which was the law of gravitation. On closer examination, it was found that they did not move quite in the manner expected. It was found, however, that by postulating the existence of another planet, Neptune, order could once more be restored. Of course, an alternative method would have been that of changing the law of gravitation. This would have involved a hopelessly complicated adjustment, and the fact that the postulation of Neptune provided a relatively simple adjustment caused mankind to accept that postulate. Following the calculations, Neptune was looked for and found; in other words, the adoption of one criterion for its existence was followed by the discovery that another already accepted criterion, that of seeing it, was satisfied. However, even had Neptune

been made of invisible substance so that no one could ever see it, there would have been meaning to the postulation of its existence as a coördinator of the planetary motions. If I asked you to believe that Neptune is real on this criterion alone, I can not demand that you accept the criterion. Your acceptance or refusal must depend upon your own psychology. If, like St. Thomas, you must touch to believe, I have nothing to say to your objections. This does not prevent me from adopting the gravitational criterion as my own evidence of the reality of Neptune; and if you worry me too much with objections I shall be perfectly willing to present you with the word "reality," or with my share of it for such enjoyment as you may obtain from its contemplation, confident in the fact that whether I call Neptune real or not will have little effect upon my calculations. My nautical almanac will be well prepared to give the story of the things upon whose existence we both agree, in spite of what you may think of my halucinations with regard to its origin.

Following the reorganization of scientific thought which came as a result of the work of Newton and Galileo, science concerned itself for a considerable time with the laws pertaining to matter in bulk, where questions of reality did not seem to invite very stringent attention. It is true that even in Newton's time questions as to whether light was attributable to certain hypothetical particles or to waves in a hypothetical medium were upon the board for discussion; but the knowledge of the times was not sufficient to develop all of the necessary consequences which would be demanded of the particles or of the waves, and which in their demands might have raised questions as to their reality, as they have done in recent years. It was sufficient at the time to think of some kind of a medium capable of transmitting wave-like phenomena in the same kind of way that water transmits waves or that solids transmit waves. Only in later years did the more careful analysis of the necessary properties of the medium demand that it should behave in a manner so different from any substance with which we are acquainted as to raise the question of whether it should be called real or not.

For the most part, however, men of science were concerned with the motions of the heavenly bodies, the phe-

nomena of the tides, the vibrations of air in sound, and the like; and while a conscientious philosopher might at any stage of the procedure have demanded considerations concerned with the reality of things, these considerations did not force themselves upon us. As the laws of the coarse-grained phenomena of matter became further ferretted out, however, there came a time when, in order to correlate even many of the phenomena which our eyes witnessed, it was necessary to introduce a sub-grained mechanism which was itself invisible and imperceptible to our senses in the details of its supposed actions. First, there came the rather vague concepts of atoms and molecules. We could explain the pressure of a gas contained in a closed vessel by supposing it to consist of a large number of individual particles flying about in all directions with high speed, bombarding the walls of the vessel, and producing thereon the equivalent of a steady pressure as would a stream of sand directed against the wall produce such a pressure. The fact that the pressure exerted by a given quantity of gas is inversely proportional to its volume was readily explained by this view; and by correlating temperature with the average kinetic energy of the particles, the relation between pressure and temperature was also provided for. In all of these matters, and in many of their extensions, it was not necessary to endow the particles with any properties other than that of the power to possess inertia, so that what was demanded of them was so like what was to be found in large bodies which could be seen that the demand carried no implications which invited questions as to the reality of the particles.

In the science of chemistry, a correlation of the observed phenomena demanded a richer set of properties for the particles than was required by the kinetic theory of gases; but here the properties found their expression in more or less empirical form without the invocation of any detailed form of mechanism to account for them. It was as though the chemist said: "The behavior of the various atoms and molecules can be accounted for provided that we postulate them to be things which can do this and that. Perhaps some day the physicist will find how they can do it without offending our susceptibilities; but whether he does or not, we do not

care very much. We have a means of making our chemical compounds." You see, the chemist is the lineal offspring of the alchemist, and until he became converted by contact with the physicist he was content and happy in the application of his collected receipts for the manufacture of his nostrums.

In addition to chemical phenomena, however, we became involved in increasing degree with others having to do with the kind of light emitted by incandescent gases. The spectrum of this light showed lines which were arranged in groups of great complexity but, nevertheless, of very characteristic regularity. This structure, so unambiguously evident to the eye, seemed to call for some equally unambiguous mechanism to account for it, and there seemed no place to look for this mechanism other than in the sub-grained structure of the atom itself. The first hopes were in the direction of building for the atom a vibrational structure, or a structure capable of emitting vibrations to some all-pervading medium, and founded upon such mechanisms as were already familiar to us in the observable phenomena of nature. We should have liked to see the atom working as a dynamo works, or as a watch works, or as the ripples upon a bowl of water work, or as the solar system works. Suppose we had been successful. What would have been our attitude towards the reality of these sub-grained structures which we could not see and could never hope to see? I think we should have been in the position of one who had discovered Neptune by calculation, but had no means of seeing it. I cannot tell you whether, under these conditions, it would have been appropriate for you to call the atomic structure real or not. Before I can tell you that, you must first give me your criteria of reality. I think that in this you will find some difficulty. You will start off, possibly with a feeling of irritation and in the temptation to say, "Every fool knows whether a thing is real or not"; but when I ask you to state your criteria for the reality of the parts of this mechanism, the thought may first pass through your head that you would like to smell them. After all, that is the criterion that most animals seem to apply concerning the reality of things. Realizing that this demand is perhaps inappropriate and that you must further admit that the parts of the atom are too small to see

or to feel, you will begin to talk, subconsciously to yourselves if not to me, about what the parts of the atom would smell like or look like if you could really make them big enough to have an odor or an appearance. If I hear your thoughts in the matter, I fear I shall have to disconcert you by suggesting that possibly the sense of smell is concerned with the activities of the atom as a whole, and that it would be as meaningless to talk about the odor of one of its parts as it would be to talk about the scheme of administration of the propeller of an airplane because there was sense to talking about the scheme of administration of the army of which the airplane forms a part. The fact is that in seeking to rejuvenate your idea of the reality of things, you attempt to accompany the act of magnifying the parts of the atom by the endowment of those parts with the kind of attributes which are the attributes of the atom as a whole. I fear that if you should try to state your criteria for the reality of the parts of the atom which I have envisioned, you would find that you would have to discard one by one the things which you had subconsciously felt were relevant, until finally you were left with no more than I claimed at the start, the bare properties of the parts of the atom necessary for the functions demanded of them and without extraneous adornment. Again, I say the question of whether you should now call these things real or not is one concerned with our conventions as to the use of words. You must settle that matter with the author of the dictionary, rather than with the philosopher.

It is ture that in adopting the artifice of envisaging a sub-grained mechanism, imperceptible directly to our senses, to account for large scale phenomena which do affect our senses, we are open to a possible ambiguity in that there may be more than one sub-grained mechanism which will fill the needs. One might be tempted to maintain that only one of these sub-grained mechanisms can be the real one which actually exists. I think I might devote a good part of this lecture to analyzing that contention and to supporting the view that under the conditions cited one might refer to all of the mechanisms as equally real. I must not succumb to this temptation now because I have other matters to discuss. I will return to it; but I may say that to worry about the

apparent ambiguity would be like worrying about whether it was the Emperor of France or the General in Chief of the French Armies who was unhappy after the Battle of Waterloo.

Now, in the attempts which were made towards visioning a structure for the atom along the lines I have so far cited, the parts of the atom with which we dealt did obey some of the laws which are familiar to us in the large scale things of life. In picturing a structure which was something like the solar system, with particles rotating round a central body analogous to the sun, we did not demand that those particles have upon them mountains, rivers, green fields, and other people who are also studying atoms; but we did envision that at least as regards the motions of these particles in relation to their common center there was some similarity to the form and type with which we were familiar in the large scale things of life, in astronomy, for example. However, it turned out that in our search for a theory of the atom the necessities of the facts drove us even further from the kind of structure which is familiar to us in the large scale things of nature than the naive mechanistic structure, to which I have just referred, but without specification in detailed form.

In speaking of the changes of attitude which accompanied the development of atomic concepts from those following the lines of the older mechanics to the kind of concepts envisaged today, it is perhaps well to pause and comment upon what it is that limits in our mind the arguments which are to be regarded as natural and appropriate to any field; for it must be emphasized at once that the modern theories of the atom are in no way more lacking in richness of law content or in definiteness of statement of behavior in terms of our observations than were those which represented the attempts of earlier days. Indeed, they are more explicit in the story. I exclude, for a moment, matters having to do with statistical considerations so frequently employed in modern theories; for although there is a certain indeterminism involved, this indeterminism is itself an ordered thing and is not symbolic of chaos in the mental processes of the mathematicians, but has its counterpart in the limitations of possibilities inherent in our observations.

It is an ideal to believe that the laws of all phenomena can be included under one general scheme. It is an ideal to believe that in the light of this scheme, the general principles, for example, which the artist tries to instill into the pupil whom he is teaching to paint, and the general principles of harmony, counterpoint, and the other abstruse elements of musical composition, and the principles involved in the construction of an automobile or a steam engine, are all brothers related to one another in as definite a manner as today we recognize the relationship between the mechanical performances of an aeroplane and an autogiro. In spite of this ideal, whose merit is, however, questionable, the fact is that people talk in different languages in different fields. While, as a man of science and a student of physics in particular, I must be sceptical about the artist who shows me his old violin, and tells me that it is better than any other because it was made by Antonio Stradivari over 200 years ago, because it has some of the soul of the maker in it, and because it is patterned after the form of the Virgin Mary, while I am sceptical of all this, I must believe that the owner is trying to say something which has meaning. I know him quite well. He is a very intelligent and delightful person. He is certainly no fool; and faced with the idea that the violin really sounds good and that it can fetch $50,000.00, I must be persuaded that unless I am willing to believe that all are mad who would pay $50,000.00 for it (and much madness is necessary to waste $50,000.00) there must be something in what he says. I cannot accept his reasons; but I have to accept the fact that the language he talks is the language which enables people to act in relation to the acquirement of and performance upon violins.

I have a darky chauffeur, and when anything goes wrong with the car, he seems to be able to put it right. This extends even to the principles of operation of the electrical parts. Now in giving his opinion as to what is wrong and his reasons for what he does to put them right he talks the most extraordinary set of principles of electricity that I have ever heard. Nowhere in any textbook on physics have I seen anything like them; but he applies them, and the car goes. Sometimes I am inclined to think that for that limited

field of electricity applicable to automobiles and to my machine in particular, his fundamental principles of electricity are better than mine.

And so the reasons which seem "reasonable" for the explanation of things vary very much in the different fields of culture and learning. We may contend as strongly as we wish that this should not be so and need not be so, but it is a fact that it is so. Moreover, the "reasons" for things associated with any particular field are apt to become rather sharply circumscribed, so that relatively minor deviations from the nature of the "reasons" introduce elements of artificiality to students of the field concerned, and tend to endow the unfamiliar concepts with elements of what we call unreality. With such varied reasons as we encounter for the behavior of things in different fields, it is not surprising that the natures of the reasons palatable to us in any one field itself are apt to change from epoch to epoch, from decade to decade, and even, in these times of rapid advance, from year to year. The abstruse things of today become the common-sense things of tomorrow. With pains and pangs does the mind sometimes follow these changes; for the mind is what we may call a progressively conservative thing; and it is apt today to become extremely conservative in its radicalism of yesterday.

And so in the minds of those scholars of the last century who concerned themselves with the realm of the atom, the train of thought which was "reasonable" was one in which the story of what happened was told in the motions of things, the story of these motions themselves being told in terms of the relationships of the things which moved to other things in the structure and to one another. The motions of the planetary bodies at any instant were described in terms of their positions in relation to the sun. That which happened to the motion of a planet at any instant could be calculated from its position in relation to the sun. The concept was that of the sun being responsible for the motion of the planet. In the customary language of the times, and even of today, for that matter, it was one in which the sun was said to produce a force on the planet, which force was responsible for the change of motion occurring in the planet at that instant.

Much thought was expended even in this matter for the purpose of realizing some kind of continuity of medium between the planet and the sun by which the force could be transmitted in such a manner as to make it more analogous in its behavior to the forces which are transmitted through a piece of elastic when it is pulled, or through a rod when one end is pushed. It is quite true that even the transmission of forces through rods was not something without further need of clarification. Speaking of cohesion, which is the property involved in the transmission of this force, Sir Oliver Lodge once remarked that it was still an inexplicable thing as to why, when one end of a rod was pushed, the other end moved, to which the celebrated comic periodical "Punch" replied that it was an equally inexplicable thing as to why when one end of a man is trodden upon the other end shouts.

So the ideas of astronomy sought a place in the atom. There were things which moved—electrons, and the like—and it was sought to express their motions in terms of their relative positions in relation to one another. It was hoped that the motions would tell the necessary story if we caused the atom to be pictured as existing in an all-pervading medium, an æther, so that the moving parts transmitted some of their motions to the æther, which then carried the story to all the other atoms in the universe, including those of our measuring apparatus, and those of our own substance. In the picture created, much of what constituted the naive concept of reality had already evaporated. We had become content to seek in the particles which constituted our atom no properties other than were required for their functions. We were not troubled as to the color of the electron or as to the question of what it was made of. We had become satisfied that those questions really had no answers. The æther gave trouble for a long time, because it seemed so very unreal. We were content not to worry about such questions as to what its boiling point might be, and as to whether in the extreme cold of interstellar space there might be icebergs of æther moving about, since everything else would freeze at such temperatures. We were content to avoid spoiling it by forcing upon it all sorts of properties extraneous to its functions simply because ordinary matter had those

properties; but the thing that made it seem so very unreal was the fact that even the properties it *had* to have were so very different from the properties that anything else which we knew had that it was hard to accommodate it in the realm of respectable realities. It was not even a respectable ghost; for though a ghost lacks much of the substance of a living being, he at least seeks to clank his chains in some sort of imitation of what he would do if he were alive. It was, I believe, Lord Salisbury who once defined æther as "a word devised to provide a nominative case to the verb 'to undulate.'"

Even with all the foregoing concessions in respect of freeing the necessary elements of the atomic world from too many of the embarrassing characteristics which we had been accustomed to associate with reality, it still refused to work properly. The motions of the parts of the atoms didn't do the right things and the æther didn't transmit the story in the right way, so that towards the end of this period of thought, I was constrained to give another definition of the æther which I hope you will pardon me for repeating here. "The æther is a medium invented by man for the purpose of propagating his misconceptions from one place to another." This was in the prohibition era so I added the observation that "Of all subtle fluids invented for the stimulation of the imagination, it is the only one which so far has not been prohibited."

When it appeared that the types of mechanism to which I have referred could not be strained in any reasonable way to tell the story desired, a change in the mode of thinking became inevitable. However, drastic as that change appeared at the time, it was a timid change as we see things today. The electrons and so forth in the atom were allowed the prestige of continuing to occupy it and of conforming to some of their rituals of the past, as exemplified by their obedience to the old laws of motion. However, when it became necessary for them to do anything which really mattered, it also became necessary for them to do that thing in a manner governed by no scheme of forces with actions anything like those of our previous pictures. The atom was supposed capable of existing in a number of different states, in each of

which it carried out the ritual of the older laws, but did not do anything of importance. It was supposed, however, that a kind of revolution could occur at any time which would convey the atom from one state to another. The story of the revolution itself was not contained in the atom's ritual. The atom was like a capricious coquette, who could change from the state of life of a party-going debutante to a social worker, or a domestic servant, with no other reason given than the fact that on each occasion she changed her mind. It is true that we tried to investigate what was involved in that change of mind. We were able to relate her changes of mind in part to external influences, but influences which operated according to no laws simply related to the laws which governed the ritual of her existence in any of the states into which she, from time to time, settled down. Even with this widening of possibilities as to the nature of the external influence, the external influence was not enough, and a goodly element had to be left to her own capriciousness which defied all attempts at harmonization in reason.[1]

As we contemplated the atom in this stage of development of our theories, we saw a rather patched affair. The old rituals, which we had formerly hoped would accomplish everything, accomplished little but the provision of states of dignity, and the interesting things were done according to the new principles. As we came to look closer into the matter, we came even to doubt whether these old dignitaries which occupied the atom of our old picture were really now doing anything at all but look on and maintain as far as possible the atmosphere of older times to give man comfort in, alas, the false belief that the new things were being done under the authority of the old regime. The atom was something like an old government institution with a lot of old gentlemen who are really due for retirement hanging around in apparently important positions because nobody has the heart to discharge them, doing really nothing and not even controlling the policies of the organization any more, but extremely meticulous about the appointments of the

[1] Physicists will recognize that these changes of states of existence and the chances of their occurrence are symbolic of what is involved in the spontaneous and induced Einstein probability transitions in the case of the atom.

smoking-room, the maintenance of the architectural form of the building, and the preservation of certain traditional functions which spoke the glories of the times when they themselves were in action.

If now you ask me whether these old denizens of the atom were to be regarded as real, I have to answer in something like the manner in which I would answer the question of whether these old gentlemen to whom I have referred were really the functionaries listed in the official record. I should have to say: "Well, Mr. X is nominally head of the department you are speaking of, but Mr. Y is the actual head. Mr. X really doesn't do anything." Even this analogy is a little too strong in favor of Mr. X, as a representative of his counterpart in the atom, because Mr. X continues to have certain attributes which, while not part of his official functions, nevertheless enable me to recognize his existence. He has a nice white beard which I can see. He has a nice polished bald head which performs the function extraneous to the activities of the department of reflecting the light of the sun with great intensity. No, if I am to use Mr. X as my analogue of some of the things in the atom, I must ask you to allow me first to kill him. Then I shall say: "The spirit of Mr. X is still felt in this organization. His deeds will never die. His name is immortal, and his influence is here today." Now, if you ask me whether the ghost of Mr. X is real or not, you ask me the same kind of question that you ask when you demand to know whether some of those concepts in the atom of which I have spoken were, or I may even say are, in actuality there.

Now the modern age differs from that of the intermediate age in the acquirement of a greater ruthlessness. The rituals performed by the old gentleman in the atom took up too much space. They interfered with too many of the necessary functions, and they have had to be abolished. The activities of the atom have been reformulated with a view only to their efficiency as regards the work to be done. There still remain some ghosts of the old picture. There was some truth and meaning as well as extraneous meaningless adornment in the atomic organization at the period in which I have likened it to the office peopled by the old gentleman and the younger workers aforesaid. In the newer school, however, we have

tried to dissect out the essential elements and cut off everything which is an encumbrance. With the extra room and freedom from the constraint of the old rituals provided in this way, we have been able to enrich the harmony of relationships between the different things with which the atom is concerned. There still remains the concept of what I may call various states of dignity in the atom. The young lady of my former analogy still exists sometimes as a party-going debutante, sometimes as a social worker, and sometimes as a servant. We still have no "reason," *in the old sense*, for her changes of mind which take her from one to the other. We have, however, come to realize that there never could have been any reason, in the old sense, for these changes. (I apologize to the ladies for the femininity of my illustration. I beg you to believe that the more I dematerialize the atom, and make its laws more ethereal, or may I say spiritual, the more I find it necessary to endow it with feminine characteristics—the bad with the good.) However, while we have given up seeking a reason for the changes of mind of the lady concerned in terms of our old laws of forces, or what in my analogy I may call "rigid parental control," we have come to some success in being able to predict what the good lady is likely to do. The laws of this prediction are founded upon what I may call "laws of temptation." In any one state of the lady's existence—in any one state of the atom—there is a temptation to change to any one of the other possible states of existence, and we have discovered how to calculate the extent of the temptation.

I am getting a little mixed up between the old gentlemen and the young ladies. However, if you recall my language, you will realize that the young lady has been represented rather as the atom as a whole, without any implications as to materialism of its parts. This young lady, the atom, is the goddess to whom we pray to give us an answer to what we may expect when we look at something which is very different from an atom, an exploding star, or when we look at the aurora borealis, or at some instrument in our laboratories which is said to be recording something about cosmic-rays. On the other hand, the old gentlemen were symbolic rather of those thoughts of material things of which the atom was, in former days, supposed to be made. Even today, the relics of these

old gentlemen are left. As I have said, their former rituals of the intermediate stage of development have been abolished, not because they did anything, but rather because they got in the way of what was to be done. However, names still hang about the office of the atom. Only the portraits of the old gentlemen are on the walls, but when the atom is allowed to do anything in our mathematical calculations, we still, as a matter of courtesy, go to the room of the old gentleman who in the old days tried to do that thing, we bow to his portrait, and send out the order for action, telling the world that it came from him. Or, to return to the reality of expression, if not of things, when a quantum of light comes out from an atom we say that an electron sent it out, although, if we trace in our thoughts the mechanisms of our calculations, we shall find that the electron did no more than was done by the portrait of the old gentleman to whom I have referred.

Not only in the realm of what we formerly called "things" has the significance of reality passed in the last few decades through the rigid fire of critical examination, but in the realm of concepts also the like has occurred. There was a time when, in making the statement that a body was absolutely at rest in space, we said something which we thought had meaning. There was a time when the time interval between two events, the explosion of two stars in the heavens, for example, was supposed to have absolute meaning, regardless of who measured the time intervals concerned. Of course, if I measure the time interval and the stars are at vastly different distances from me, I am constrained to think that in order to get correct measurements I must allow for the fact that the light from one star took longer to reach me than did the light from the other. In former days, there was a kind of feeling that when this correction was properly made we should arrive at a unique meaning for the absolute time interval between the events. It turned out, however, that nature seemed to work in such a way that if I should make this measurement, correct it for light velocity, and arrive at this time interval between events, and if somebody else, moving relatively to me with constant velocity, should do the same thing, the results for the much desired true time interval would be different. According to the old view of things, if

you formed the old concept to the effect that I was at rest, you would readily seek to explain the difference by saying that the other man was in motion and, therefore, used the wrong value of the velocity of light in making his correction for time lag between the stars. However, a more careful analysis of the situation, made on the basis of the facts of experiment, led us to conclude that there is nothing that I can do to prove that my measurements and calculations are correct that the other man cannot do to prove that his are correct. When the attempt is made to resolve this apparent paradox, one comes to realize that there was nothing to the intuition which we formerly had as to there being an absolute meaning to the time interval between the events, and it turns out further that there was no meaning to be attached to the absolute velocity of a body in space.

Time, at least as you measure it, will not permit that I extend this lecture into a discussion of the principles of relativity. I may perhaps cite a very crude analogy which will perhaps give to those unfamiliar with the subject some comfort to counteract what may otherwise appear to be a terribly paradoxical inconsistency of language. Let us suppose, for convenience, that no musician had what we call absolute pitch. Under these conditions, play a tune to a musician. Now play the tune again in another key. I ask you which was the right tune. You would have to answer me that there is no meaning to one being right and the other wrong, both are equally right. Suppose I asked you which was the right pitch for the tune, and suppose you had no basis of finding out except from the tune itself. Since both tunes would be to you exactly alike, all that you would be able to say would be that they were in different keys, but you could not say which was the right key, and in fact, could not give any meaning to the word "right." Now, it is true that musicians do have absolute pitch, but that will not destroy my analogy. In that analogy, pitch shall be analogous to velocity through space. The tune to which I referred shall be analogous to an experiment which we do. The totality of all possible tunes shall be analogous to the totality of all possible experiments. The fact that there can be, in different keys, two similar tunes which sound exactly alike shall be analogous

to what the principle of relativity maintains to the effect that for every experiment performed in one system there can be another performed in another system moving with constant relative velocity, the two experiments giving results which, to their respective observers, are exactly alike. Just as, from the tunes under the conditions cited, you could not attach a meaning to absolute pitch, so, from the interpretations of the experiments in the other example, we find we cannot give a meaning to absolute velocity.

Earlier in this address I introduced you to our sad professor of English who was in possession of a word from which we had taken the meaning; and we promised to give him a meaning for his poor word. And so I tell you that the things which I shall call real are the principles which, in the ultimate analysis of things, are found to have the properties of harmonizing the phenomena of the universe as our senses reveal those phenomena to us; and if our professor asks for something more substantial through which the principles may operate, if he must think of things rather than of ideas, I must beg of him to remove from his consciousness the desire for the vague thing he calls substance, and I must ask him to think of the atom more as I have envisaged it above, as a goddess with rules of conduct and procedure. I must ask him to believe that the part of his desired picture which is concerned with his intuitive thoughts of reality, and to which, in illusion, he has attributed the atom's potentialities, is just the part which, in the last analysis, has no meaning as applied to the goddess. It is an idol, and our beloved professor who has worshipped it is a heathen.

Throughout the world's history man has concerned himself with seeking the explanations of things in terms of intangible deities whose decrees were the beginnings of things, and which themselves called for no reason for their origin. Man was content to begin with the decrees of the gods. And now again today, the man of science must return as to the gods; but no longer are these gods capricious beings with attributes founded upon a supposed likeness to himself. The man of science today learns the habits of his gods by the old biblical instruction as applied to other beings: "By their fruits shall ye know them." He seeks to find what the gods must be that they may do that which they do.

In ancient times, and even today, there were many different concepts of the gods, all designed ultimately to fill the same role. And so in science today, while we are content if we find one set of principles which harmonizes the universe, we have no absolute guarantee that there may not be more than one set which harmonize it equally well. There may be many sets of gods which may be regarded as governing nature; and I should like to use the word "real" for whom our professor of English is seeking a job, to describe equally well any one of these sets of principles, any one of these groups of gods. You can have whichever group you wish. They are all real. They are like different instructors whom you call in to tell you the story of some particular art or branch of learning. They all tell the same ultimate story, but in different ways. And if you object to my calling all of these groups of gods real, and say that only one of them can be, in actuality, real, I will bow to your desire in part. I will claim for them that, as far as anything concerned with them can ever be found out, they all have the characteristics which you somehow feel can be the characteristics of only one set of them; and so I will cater to your desires to the extent of calling them all "realistic."

I shall, in fact, regard as real that set of principles which fits the facts. If there is more than one set, I shall be undisturbed. Even you, who do not like the use of the word "real" for all of these sets of principles, will probably be more content with the designation "realistic"; and we will make that word our starting point, and we will suggest for our professor of English a definition of the word "real" to the effect that it is a special form of the word "realistic" applicable when there is only one candidate for the designation.

In spite of all I have said, you may maintain that you cannot escape the very emphatic consciousness of the substance of things. You will tell me that when you bang your head against the wall, it seems most emphatically evident that there is reality in the wall and that the wall is there. You will protest that all things are not shadows. You will tell me that you can touch things, you can feel things, and that deep down in your very bones you believe that they are there. I sympathize with your feelings, but I must warn you that all you have as your criterion for the existence of whatever it

may be that you are thinking about, all you have for your criterion of the reality of this "substance" is the set of impressions you get from your senses; and my goddesses of the atoms, those principles which I have said are the things ultimately real will, if they have been properly chosen, reincarnate in the approximate and crude form of your senses that concept of reality with which you started, and which seemed so very substantial and meaningful to you. They will reconstruct this reality, not with simplicity, but with the appropriate complexity which should adorn it. I fear there will be many things about it of which you will feel rather ashamed; but, humbled in its reincarnation, it will nevertheless be surer of the ground left to it. When the goddesses have reconstructed for you the meaning of the reality of my presence here this evening, you will not be so disturbed when confronted with the alarming doubts which I cast upon this matter at the beginning of my address and concerned with the fact that your evidence as to the presence of what you call this three-dimensional being who is addressing you came through two-dimensional images upon the retinas of your eyes, images which occurred twice, once in each eye, images which were upside down, and which conveyed their message to your thoughts by a process in which what the right eye sees the left hand side of the brain interprets.

And so we begin to see that, in line with the spirit of this philosophy, there is an artificiality in the old reality, and a true reality in the new artificiality. The new reality is not a development or an enhancement of the principles of the old; but the old is rather a rickety, somewhat illogical, vague, and incomplete structure which comes nearest to respectable form when reincarnated out of the principles of the new.

If, in spite of all I have said, you still seek to prune your own concepts into something which, over the whole realm of nature, has those elements of reality desired by your primitive senses, then I fear you are doomed to disappointment. At best, you are doomed to the life of one who seeks an ideal which becomes more and more remote as he continues to pursue it. Like the moon, the ideal will always seem to lie behind the mountain ahead; or, like the rainbow which seems to descend to the earth at some definite spot, it will move

away from you as you approach the spot where it seemed to be. It is said that God fashioned man in His own image; but it is very evident that it is man who has fashioned God in his image. If, in your search for that which the imp in your soul has told you is final and which is reality, you should beguile some sorcerer to bring your ideal to you for a moment for your inspection, you will see but the image of your own face in the picture. You will see the mechanism of that thing which was the old reality fighting like an octopus to find holds for its tentacles upon the true realities of nature; and as you examine more closely the form and actions of that octopus you will find that it is none other than your own brain.

THE EVALUATION OF NEGATIVE FILM SPEEDS IN TERMS OF PRINT QUALITY.*

BY

LOYD A. JONES.

(*Conclusion*)

IX. STATISTICAL DETERMINATION OF PARTICULAR ASPECTS OF PRINT QUALITY.

The next phase of the work deals with the establishment of the correlation between negative exposure and certain specific aspects of print quality. These series of prints were inspected by a large group of observers who were requested to choose prints exhibiting certain characteristics. Referring again to the qualitative curve of print quality, Fig. 26, it is apparent that quality changes quite rapidly up to a negative exposure corresponding to A. After this, further increase in negative exposure produces *little, if any*, further improvement in print quality. The print corresponding to the negative exposure of A will be referred to as the "first excellent print."

First Excellent Print.—The first excellent print, therefore, is one in the series corresponding to the minimum negative exposure which gives *almost maximum* and possibly *maximum* print quality.

Just Acceptable Print.—It seems desirable, also, to establish one other aspect of quality, namely, that which may be expressed as "just acceptable." Such a print is not as good as the "first excellent" print, nor as bad as the worst of the series, but lies somewhere between the two, and is a print which might be worth saving, provided a better print could not be obtained. It is difficult to define precisely the criterion of "just acceptability" but the frequency with which a certain quality has been chosen by a large number of observers as being just acceptable in itself proves that there has been established in the minds of these observers a fairly definite idea of "just acceptable." It has been suggested that this criterion might be explained as follows: Suppose a person, who might be referred to as an average amateur, takes a roll

* Communication No. 683 from the Kodak Research Laboratories.

of exposed film to a finishing station and instructs the finisher to develop the negatives and make prints from all that are "worth printing"; a just acceptable print would be one having the least quality for which the customer would pay.

Print Judgments.—In Table III are shown the detailed results of the print judgment by the first thirty observers. At the bottom of the table in the line "Average X" are the averages of the print numbers chosen by this group. The values in this table serve to convey an idea of the magnitude

TABLE III.

Print Judgments

Observer	Just Acceptable				First Excellent			
	A	*B*	*C*	*D*	*A*	*B*	*C*	*D*
1	3.0	4.0	4.0	5.0	5.0	7.0	6.0	8.0
2	4	4	5	5	7	7	8	8
3	2	3	3.5	4	9	9	8	8
4	2	3	4	5	5	6	5	8
5	2	3	4	5	5	6	8	8
6	2	2	3	4	5	6	6	8
7	3	3.5	3	5	6	7	7	8
8	4	4	5	5	7	6	8	8
9	2	3	4	5	5	6	5	7
10	2.5	3.5	4	4.5	6	7	7	7.5
11	3	4	5	6	5	6	6	7.5
12	4	4	5	6	8	7	8	7
13	4	4	5	6	6	7	8	8
14	2	2	3	4	6	7	6	9
15	1.5	2	3	4	5	6	7	8
16	1.5	2	3	4	4	5	7	7
17	2.5	2.5	4	5	8	8	7	10
18	2	2	3	4	7	8	7	7.5
19	4	3	4	5	6	8	7.5	9
20	2.5	3.5	4	5.5	5	6.5	6	8
21	2.5	3	4	5	4	5	7	7
22	2	2	4	5	5	6	7	9
23	2	3.5	4.5	5	5	6	8	8
24	3	4	4.5	5	9	7	11	8
25	3	3	4	5	9	9	11	8
26	3	4	5	6	5	6	6.5	8
27	2	3	4	5	8	8	9	9
28	4	4	5	6	6	6	7	8
29	3.5	4	4.5	6.5	9	9	8	9
30	2	3	4	4.5	9	6	7	8
Average X	2.7	3.2	4.1	5.0	6.3	6.8	7.3	8.0
Average Y	2.8	3.2	4.0	4.9	6.2	6.5	7.1	8.0
Average Z	2.8	3.2	4.0	4.9	6.2	6.6	7.2	8.0
Average S	2.9	3.2	4.0	4.8	6.5	6.8	7.5	8.2
Average T	2.8_5	3.2_0	4.0_0	4.8_5	6.3_5	6.7	7.3_5	8.1

of the spread in print numbers chosen by different individuals. In the line "Average Y" are shown averages derived from the judgment of these prints by seventy additional observers, and in the next line of the table, "Average Z," will be found the mean value for the entire one hundred observers. It should be noted that there is very little difference between the average based upon thirty observers and that including the entire one hundred.

When the series of prints had been judged by the one hundred observers mentioned above, it was considered that the values of negative exposure yielding "first excellent" and "just acceptable" prints had been established with sufficient certainty to warrant using them for the evaluation of the effective speeds of these materials in terms of print quality. The long and laborious computation of tone reproduction quality and the attempt to establish a satisfactory correlation between sensitometric results and the statistical results derived from the print judgments was begun. However, it seemed desirable to continue the accumulation of statistical data in order to determine whether a different group of observers might possibly choose appreciably different negative exposures as yielding the "first excellent" and "just acceptable" print qualities. The average for a second group of one hundred observers is shown at the bottom of Table III in the line "Average S." It will be seen that the second group chose almost exactly the same negative exposure for the "just acceptable" prints. They have, however, asked for a slight increase (about 10 per cent) in the negative exposure yielding the "first excellent" prints. It is interesting to note that the amount of increase in negative exposure is practically identical for the four materials and hence any evaluation of speed in terms of this negative exposure will result in *relative* values which are practically identical with those based upon the choices of the first one hundred observers. In the last line of the table, "Average T," are shown the averages for the entire two hundred observers. All subsequent computations and the correlations which will be discussed later are based upon the "Average Z." The differences existing between the average Z and average T are so small as to be of little significance.

In Table IV are shown the number of times each print was chosen to represent the two criteria under investigation. In case an observer had indicated the choice of a print halfway

TABLE IV.

The Number of Times the Various Prints Were Chosen as Fitting the Two Criteria as Indicated, Two Hundred Observers

First Excellent

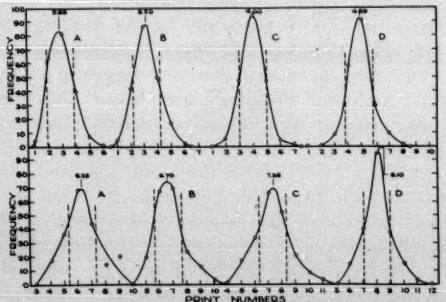
	1	2	3	4	5	6	7	8	9	10	11	12
A			0	17	36	68.5	44	14	20.5	0		
B			0	1	19.5	68	70	24	13.5	2.5	1.5	0
C				0	17	33.5	66	52	20.5	7	4	0
D					0	14.5	38	94	36	14.5	3	0

Just Acceptable

	1	2	3	4	5	6	7	8	9	10	11	12
A	4.0	65.0	82.0	40.5	7.5	1	0					
B	4.0	41.5	86.0	50.0	16.5	2	0					
C	0	5.0	47.0	94.0	47.5	4.5	2	0				
D	0.5	0.5	8.5	58.5	91.0	29.5	9.5	2	0			

between two of the actual prints, a half point was assigned to each of the two prints between which his choice fell. This accounts for the figures to the right of the decimal point in the table. These numbers, when plotted as frequency distribution curves (Fig. 30), indicate that the spread is relatively

FIG. 30.



Curves showing the frequency distribution of print choices.

small, the majority of observers agreeing within quite narrow limits on the negative exposure resulting in just acceptable and first excellent prints. The number of choices, expressed as a percentage of the total number of observers, falling within ± 0.15 (log negative exposure) may be taken as an index of agreement. These values are shown in Table V. The

TABLE V.

Per Cent of the Print Choices Lying within a Range of ±0.15 log E

	Just Acceptable	*First Excellent*
A	72	58
B	69	65
C	73	59
D	73	67
Mean	72	62
Mean	67	

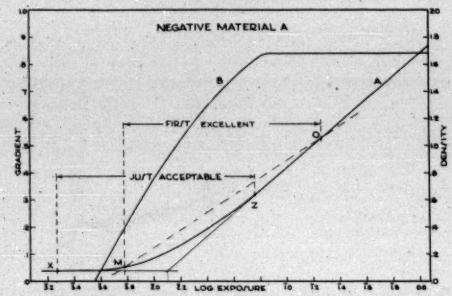
minimum is 58 per cent and the maximum 73 per cent, the average of all as shown in the bottom line of the table being 67 per cent. Thus, two-thirds of the observers chose prints made from negatives for which the exposure varied by ± 0.15 in log *E* units. This is a relatively small range, especially when it is considered that the maximum exposure is only twice that of the minimum, corresponding to a change in setting of the lens diaphragm from, for instance, f/11 to f/8. This agreement of the observers in choosing the just acceptable and first excellent prints is extremely satisfactory and establishes with great certainty the negative exposure required to give prints corresponding to these criteria.

Location of First Excellent and Just Acceptable Prints on the D-log E Characteristic of the Negative Materials.—The curves in Figs. 31 to 34, inclusive, show what parts of the *D*-log *E* curves of these negative materials were used in making the negatives from which, in the opinion of the first one hundred observers, the first excellent and the just acceptable prints were made.

Figure 31 is for negative material *A*. The curve designated as *A* is the *D*-log *E* characteristic as determined from the sensitometric strips exposed in the camera along with the test negatives. The portions of this curve lying between the

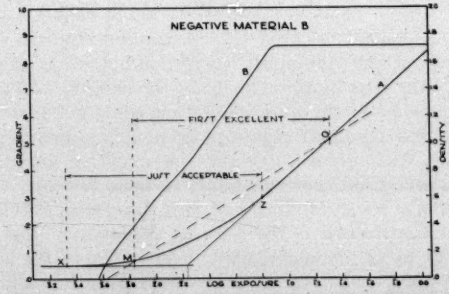
points M and O are those required for a hypothetical negative having a number 6.2, the position of these points being determined by interpolation from actual measured values on negatives 6 and 7. The portion of the curve lying between

FIG. 31.



Curve A, the D-log E characteristicf or negative material A with points indicating the positions used for making the first excellent and just acceptable prints. Curve B, the first derivative of curve A.

FIG. 32.



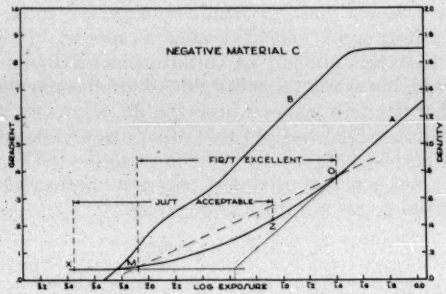
Curve A, the D-log E characteristic for negative material B with points indicating the positions used for making the first excellent and just acceptable prints. Curve B, the first derivative of curve A.

X and Z is that required for a negative having a number 2.8, this being the average for the just acceptable print. The curve B is the first derivative, $dD/d \log E$, of the curve A and its significance will be discussed later.

Figures 32, 33, and 34 give the same information for the other three negative materials.

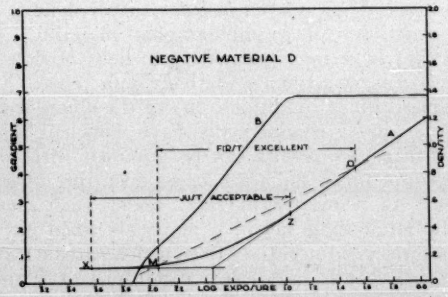
The first *excellent* print is derived from a negative which uses practically the entire toe region of the characteristic

FIG. 33.



Curve *A*, the *D*-log *E* characteristic for negative material *C* with points indicating the positions used for making the first excellent and just acceptable prints. Curve *B*, the first derivative of curve *A*.

FIG. 34.



Curve *A*, the *D*-log *E* characteristic for negative material *D* with points indicating the positions used for making the first excellent and just acceptable prints. Curve *B*, the first derivative of curve *A*.

curve. The point *M* in all cases falls at values of very low gradient and at an exposure value considerably less than that of the inertia point. It will be observed that the amount by which the point *M* is displaced to the left from the inertia

point varies from approximately 0.3 (in log E units) for material A, up to approximately 0.7 for material C. This explains at once why the inertia fails to provide a satisfactory criterion of effective camera speeds. The density value (above fog) at the point M varies from 0.03, in the case of material C, to 0.08 for material B. Thus, it is obvious that a fixed low value of density cannot be expected to be a satisfactory criterion of effective camera speeds. It may be thought that this amount of variation in minimum density is not great, but when the low value of gradient in the region of the characteristic curve where the M points are found is considered, it will be realized that a variation in density from 0.3 to 0.8 corresponds to a fairly wide variation in the value of exposure, and hence, if used as a criterion of effective camera speed, would result in rather large errors.

X. EVALUATION OF SPEED.

Before an intelligent choice can be made of a suitable criterion in terms of which to express the speed of a photographic material, a definite conclusion must be reached as to what a speed number should indicate. It is obvious that the term "speed" is related in some rather definite manner to the sensitivity of a photographic material. Thus, a material that is extremely sensitive to light obviously must be referred to as one having a relatively high "speed." Under the vast majority of conditions, any of the amateur negative materials available commercially have sufficient sensitivity so that satisfactory results can be obtained with relatively short exposure times and with lenses operating at moderate apertures. As a matter of fact, under such conditions, practically all of these negative materials when given identical exposures yield negatives from which satisfactory prints can be made. It is only when conditions of illumination are poor or when for some reason it is desirable to use extremely short exposure times or lenses of very small aperture ratio, that the question of high speed is of any real importance. Upon what characteristic of a negative material, then, should an evaluation of its speed be based?

As a beginning it might be said: *The speed of a negative material should be expressed in terms of the exposure required to*

produce a good negative. It becomes necessary then to define a *good negative.* Hurter and Driffield defined a perfect negative as one in which the opacities are directly proportional to the brightness of the object photographed. In the light of present knowledge of tone reproduction theory, however, it is known that a perfect negative as defined by Hurter and Driffield, under certain conditions of printing, may fail to produce as good a *print* as can be obtained from a negative in which there is some departure from the direct proportionality between opacity and object brightness. It is possible and probably desirable to eliminate from the definition of the speed of a negative material all mention of the characteristics of the negative. As a matter of fact, it seems logical to take the position that the characteristics of the negative may be *anything* so long as a satisfactory positive can be made therefrom. For instance, a negative might be completely opaque or completely transparent, failing to show any visible departure from perfect uniformity of density, and still be capable of yielding a perfect print on a positive material having the proper characteristics.

Returning then to a formulation of what speed should mean, it might be said: *The speed of a photographic material should be expressed in terms of the exposure required to produce a negative from which a satisfactory print can be made.* But even this is not sufficiently specific and it does not result in a single unique value of speed for a given material. With many photographic materials (assuming an object of average brightness contrast), the exposure may be varied over a wide range of values and still yield negatives from which prints of *very good* quality can be made. Moreover, on the basis of the above definition, the vast majority of commercially available negative materials might be assigned identical speed numbers. This is certainly true of the four negative materials being considered.

Finally, it seems logical to conclude that in the amateur field, at least, the formulation should be as follows: *The speed of a photographic negative material should be evaluated in terms of the* MINIMUM *exposure which will yield a negative from which a satisfactory print can be made.*

For the purpose of this discussion the above definition of effective camera speed will be adopted.

It is necessary now to decide how the term "satisfactory" should be defined. In Figure 17 is reproduced a print from negative No. 4 on material *C*. According to the statistical data, this print is just acceptable. Below the print will be found the tone reproduction curve. An inspection of the print shows it to be a rather flat smoky reproduction with practically no visible detail in the extreme shadows and very little and unsatisfactory detail rendering in the lower half-tone region. The reproduction curve shows that for about one-third of the tonal range the gradient is extremely low. If the effective camera speeds of negative materials are defined in terms of the just acceptable print, it must be realized that this necessarily means that the evaluation is in terms of prints of poor photographic quality and poor tone reproduction characteristics.

An inspection of the print from negative No. 7 (reproduction of which is shown in Fig. 20) shows that excellent photographic quality has been obtained. Not only is this revealed by a direct inspection of the print, but this judgment is substantiated by the accompanying tone reproduction curve. Good rendition of detail is obtained throughout the major portion of the tonal range with relatively little compression in the highlight and shadow regions. *Loss of quality occurs rapidly* when negative exposure is decreased below that used in making negative No. 7.

It seems quite certain that the curve in Figure 26 represents a true picture of the general relation between negative exposure and print quality. The section of the curve between *B* and *A* is undoubtedly steep. To the right of *A*, according to our most recent statistical determinations, the curve seems to follow a slightly wavy course, oscillating above and below a straight line practically parallel to the *X* axis. Usually, one or two prints made from negatives having received greater exposure than that for which the first excellent print was made are found which show *slightly* better photographic quality than the first excellent print. The difference, however, is very small as compared with the difference, for instance, between the just acceptable and the first excellent print. If

the effective camera speeds of negative materials are defined in terms of the *first excellent* print, it follows that this evaluation is based upon prints having a quality almost as good as the best possible, and if this criterion be adopted, the definition of effective camera speed takes the following form: *The speed of a photographic negative material should be evaluated in terms of the* MINIMUM *exposure which will yield a negative from which an excellent print can be made.*

Statistical Evaluation.—The most direct method of making the evaluation as defined above appears to be by the direct judgment of print quality by a sufficiently large group of observers so that the average may be relied upon to agree with the average opinion of the photographic public at large. This material has been obtained and reported in previous sections. Table VI shows values derived from Figures 31 to 34, inclusive. In the first section those relating to the negatives from which the first excellent prints were made are given. In the second column are the log E values corresponding to the point M which represents the lowest object brightness (deepest shadow). In the third column these are shown as exposures, and in the fourth, as reciprocals. These reciprocals, therefore, are the *absolute* effective camera speeds of these four negative materials. In the fifth column these are shown relative to material A as 100. In the second section of the table the same values relative to the negatives from which the just acceptable prints were made, are shown, the column designations being the same except that the numbers are based upon the exposure corresponding to the point X. If a just acceptable print should be considered as satisfactory for the evaluation of effective camera speeds, the numbers in the fourth column would be obtained. In the fifth column these are shown relative to material A as 100. In the sixth column of the first section are shown the ratios of the "just acceptable speed" to the "first excellent speed." It is interesting to note that this ratio is practically the same for *these four materials*. This means that the *relative* effective camera speeds of these four materials will be the same whether evaluated in terms of "first excellent" or "just acceptable" print quality. It does not follow that the ratio will be constant for other photographic materials that may have characteristic

curves differing considerably in shape from those used in this investigation, and, in fact, some evidence is available which indicates that the ratio may vary appreciably. If the ratio of "just acceptable" to "first excellent" speed numbers were sufficiently constant for all materials, it would eliminate the necessity of deciding between the use of these two criteria in establishing *relative* speed numbers. But it will still be necessary to decide between them in the establishment of *absolute* values of effective camera speed, and this must be done for some purposes.

It should be emphasized that the values of speed given in Table VI are based entirely upon direct experiential evidence;

TABLE VI.

Effective Camera Speeds, Absolute and Relative

	Neg. No	Log E_{min}	E_{min}	$1/E_{min}$	$1/E_{min}$ (Rel.)	R
			First Excellent			
A	6.2	$\bar{3}.78$	.00603	166	100	3.2
B	6.6	$\bar{3}.84$	.00692	144	87	3.2
C	7.2	$\bar{3}.93$	.00851	118	71	3.1
D	8.0	$\bar{2}.05$	.0112	89	54	3.1
			Just Acceptable			
A	2.8	$\bar{3}.27$	.00186	540	100	
B	3.2	$\bar{3}.33$	.00214	470	87	
C	4.0	$\bar{3}.45$	.00282	360	67	
D	4.7	$\bar{3}.56$	.00363	280	52	

no sensitometric operations are necessarily involved in their determination. While sensitometric curves have been used as a matter of convenience and for illustrating certain points, the numbers themselves can be obtained without the aid of any sensitometric technic. For instance, all that need be known about the negatives, in order to determine the speed values already given, are the exposure values for the various negatives corresponding to the lowest object brightness. This can be determined by direct photometric measurement of the brightness of the deepest shadow and the use of a formula which gives the relation between object brightness and image illumination. Thus, the values in the third column of Table

VI can be determined just as easily and precisely by such a direct measurement on the characteristic of the object and of the lens system forming the image as by the method illustrated in Figures 31 to 34, inclusive.

Sensitometric Evaluation.—While there is no question that the statistical method already described yields results of satisfactory significance, the operation of such a method of speed determination in practice is quite impossible. The amount of labor involved in making a series of negatives and prints therefrom and in having them judged by a sufficient number of observers to establish the desired certainty in the statistical average is prohibitive. It is essential, therefore, that some sensitometric method be found which will yield results in close agreement with those obtained by the statistical method. As already pointed out, speed values based upon inertia are not proportional to those given by the statistical method of direct print judgment, nor will the use of a low fixed value of density accomplish the desired end. The magnitude of the failure of these criteria to yield the desired results is illustrated in Table VII, in the first column of which

TABLE VII.

Comparison of Speed Numbers Evaluated in Terms of Various Criteria

	First Excellent Print	Inertia	0.1 + Fog
A	100	100	100
B	87	75	93
C	71	28	53
D	54	43	48

are shown the values based on statistical print judgment, in the second, those based upon inertia, and in the third, those based on a fixed value of density equal to 0.10 plus fog.

An objection, based upon purely theoretical reasoning, has already been expressed to the use of either of these criteria for the determination of effective camera speeds and the position has been taken that the one characteristic of a negative material that is of maximum importance and significance for the rendition of detail, the visibility of which is due to brightness differences, is *gradient*, $dD/d \log E$. In searching for a satisfactory sensitometric method to give results identical

with those obtained by the statistical method, it is logical to seek a correlation between some specific aspect of gradient and the effective camera speed values as determined by judgment of print quality.

From theoretical considerations of tone reproduction requirements and our visual response mechanism, it seems reasonable to expect that the minimum limiting gradient of the portion of the curve used for the making of the negative which yields the first excellent print should be related in some way to the general gradient aspects of this used portion. It seems logical to expect (as Jones and Russell * and others have pointed out) that the amount of detail loss which can be tolerated in the extreme shadow region should depend upon the amount of detail compression or expansion which has occurred in other parts of the tonal scale. For instance, in a print where a considerable compression of the over-all object contrast has occurred, more compression of shadow detail (lower limiting gradient) will be tolerated than in the case where the over-all object contrast is reproduced exactly or even enhanced. It does not seem reasonable to assume that the lower limit of usefulness should be determined by any fixed value of gradient, but that this limiting gradient should depend on what is happening throughout the whole of the tonal scale and be determined largely by the quality of the detail reproduction in the print as a whole.

In Table VIII the figures in the first column show the average gradient, $\bar{G}$, for that portion of the negative char-

TABLE VIII.

Data Showing Correlation between Minimum and Average Negative Gradient and between Minimum and Maximum Negative Gradient

$$K = \frac{G_{\min.}}{\bar{G}} = \text{Shadow Detail Compression Factor}$$

	$\bar{G}$	$G_{\min}$	$G_{\max}$	K	$\Delta K(\%)$	$G_{\min}/G_{\max}$	ΔR %
A.....	0.64	0.200	0.85	0.312	4.0	0.236	21.0
B.....	.61	.195	.86	.320	6.7	.226	16.0
C.....	.45	.125	.86	.284	5.4	.145	25.6
D.....	.48	.135	.78	.282	6.0	.173	11.3
Mean....				.300	5.5	.195	18.5

* See reference 4, page 407.

acteristic used in making the negatives from which the first excellent prints were made. These are the average gradients for those portions of the curves between M and O (Figs. 31 to 34 inclusive). In the second column, G_{min}, are the minimum limiting gradients, these being the slopes of the curves at the points M. In the next column are the maximum gradients, G_{max}, these being the slopes of the curves at the points O. In the column designated as K are shown the ratios of $\bar{G}$ to G_{min}. It should be noted that the value of K is very nearly constant for the four negative materials, the average being 0.300. A suggested name for this ratio is "the relative shadow detail compression factor," since it denotes the amount of compression of contrast in the extreme shadow as compared with the average contrast (gradient) throughout the tonal range of the object. Values of ΔK are shown in the column thus designated, and it will be seen that these deviations are relatively small, the average being only 5.5 per cent.

In a previous communication [4] it was stated that evidence available at that time indicated a rather good correlation between the minimum limiting gradient in the negative and the maximum negative gradient. The ratio of these two quantities is shown in the next to the last column with the deviations, ΔR, in the last column of the table. It will be seen that these deviations are almost four times as great on the average as those already found for K. The correlation, therefore, is much inferior and not sufficiently good to warrant following this lead further.

The perfection of correlation between minimum and average negative gradient seems to warrant the adoption of this as a criterion of effective camera speeds, at least for the materials used predominantly in the amateur field. As a sensitometric criterion of effective camera speed *the exposure corresponding to that point on the D-log E characteristic curve where the gradient is equal to three-tenths (0.3) of the average gradient of a log exposure range of 1.50* is proposed. This log exposure range represents the portion of the negative curve required for the rendition of an object having the assumed average brightness contrast with a camera and lens system having the assumed average flare characteristics.

The *sensitometric criterion* proposed in the previous paragraph is based directly upon the results of statistical evaluation of *minimum* negative exposure resulting in a print of *excellent* quality. This sensitometric criterion is in rather remarkable agreement with Luther's findings [14f] and with the criterion proposed by Romer [10] who, following the suggestion made by Luther, has adopted as the most suitable criterion of effective camera speeds the exposure at a point on the characteristic curve where the gradient is one-third of the average gradient of a log exposure range of 1.40. Previously, Romer and his collaborators [11] had suggested that the minimum useful gradient be defined as one-fifth gamma. Luther [12, 6] and Lapeyre [13] have taken the position that the minimum useful gradient should be defined relative to other aspects of the gradient of the used portion of the characteristic curve. Luther expresses his position on this subject as follows: "I should like to say that I do not put forward any definite minimum gradient, as I consider that the useful minimum gradient is dependent both on the contrast of the printing material and also on the contrast of the object. (*Phot. J.*, 1925, **65**, 185.) Only when a 'normal object' (for example, according to Hurter and Driffield, with the ratio 1 : 32) and a 'normal' positive material are defined is it possible to accept a definite minimum gradient." *

For the convenience of those interested in the literature dealing with the use of minimum limiting gradient as a criterion of speed, a bibliography of some of the more important papers is given.[14]

Possible Variation of K with Object Brightness.—Conclusive information on the possible dependence of K upon the brightness contrast of the object is not available at present. It is quite possible that for lower and higher brightness contrasts the value of K will be *somewhat* different from that derived for the brightness range used in this work. If this proves to be true, it simply means that the effective camera speed of a given photographic negative material will be dependent to some extent upon the contrast of the object being photographed. This will mean that the photographic material in question does not have a single, unique speed

* Reference 14*e*, page 143.

value and the complete information relative to this characteristic of the material could only be given in terms of a series of numbers, or perhaps a graphic representation of the speed number and object contrast. Such a course seems quite impossible to follow, for it is necessary, in many cases, to give a single number which represents the effective speed of the material. To do this, some object contrast must be chosen for which to give this information, and it seems that the most logical object contrast to adopt for this is that of the statistical average of a large number of possible objects or scenes.*

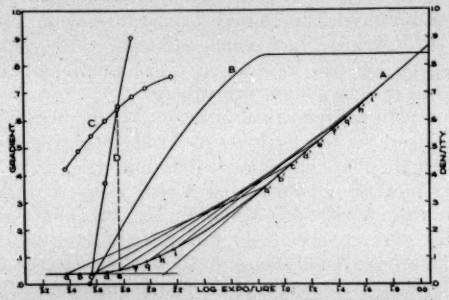
Speed Numbers Based on $G = 0.30\bar{G}$.—The average value of K, Table VIII, as derived from the statistical determination of the first excellent prints is 0.30. The values for the individual materials fluctuated from this mean. It is of interest to determine how much influence these departures from the average will have upon speed numbers computed according to the suggestion just made.

As a matter of practical utility any method which is finally adopted for the evaluation of effective camera speeds must be based upon sensitometric determinations. In Fig. 35 a graphic method is illustrated which gives a direct and unique solution for the problem of finding the point for which the gradient has any specified relation to the average gradient for a designated exposure range limited at its lower end by the point in question. Curve A is again the usual D-log E characteristic, in this case being identical to the one in Fig. 32. Curve B (Fig. 35) is the first derivative of A. In this particular case it is of interest to find the point on the curve A where the gradient is equal to three-tenths of the average gradient for a log E range of 1.50. A point, a, is selected to the left of where the slope of the curve A becomes zero, and the point a' is established so that $\log E_{a'} - \log E_a = 1.50$. The slope of the straight line drawn through points a and a',

* Since this manuscript was prepared, further statistical judgment of the minimum negative exposure required to give an excellent print when using object contrasts much lower and much higher than the brightness contrast of the test object used in this work indicates quite conclusively that the minimum useful gradient is not to any appreciable extent dependent upon the object contrast. It appears, therefore, that the sensitometric criterion suggested will apply satisfactorily to a relatively wide range of object contrasts. The detailed results of this further investigation will be published shortly.

which is the average slope of the D-log E curve between points a and a', is plotted, using the gradient scale at the left, on the ordinate passing through a. Similarly, the slopes of straight lines passing through b, b'; c, c'; etc., are determined and plotted. The curve C therefore represents the average slope for the specified log E interval ($\Delta \log E = 1.50$) plotted against the minimum log E value of each consecutive log E interval. If the ordinates of curve B be multiplied by $1/K$ and then plotted on the same gradient scale, curve D is obtained. A perpendicular dropped from the intersection point of curves C and D to curve A locates the desired point at which

FIG. 35.



Graphic construction for finding the point on the D-log E characteristic (curve A) at which the gradient is equal to three-tenths (0.3) of the average gradient and Δ log E is equal to 1.50.

the gradient of A is equal to $K \cdot \bar{G}$. By determining the exposure value corresponding to the point just located and taking its reciprocal, the speed number as based upon the criterion suggested above is obtained. In Table IX, column B, are shown the speed numbers derived in this manner and they may be compared directly with the speed numbers, column A (see Table VI, column $1/E_{min}$), which were derived directly from the statistically chosen first excellent prints. Under ΔS are shown the deviations, and in the last column these deviations are expressed as percentages. It is quite evident from a consideration of these values that the lack of perfect constancy of K does not produce any serious disturbances of the speed number values.

TABLE IX.

Data Showing Agreement between Statistically and Sensitometrically Determined Effective Camera Speed Values

	A	*B*	ΔS	ΔS (%)
A	166.0	169.8	3.8	2.3
B	144.0	150.8	6.8	4.1
C	118.0	112.7	5.3	4.5
D	89.0	84.4	4.6	4.5
Mean				3.9

XI. THE EVALUATION OF RELATIVE PRINT QUALITY.

Thus far, attention has been concentrated on certain specific aspects of print quality. For instance, the negative exposure resulting in a *just acceptable* print and the *first excellent* print has been determined statistically, by the direct judgment of a large group of observers, and a sensitometric method has been found by which the negative exposure resulting in the first excellent print can be determined. It is obvious that the first excellent print has better tone reproduction quality than the just acceptable print, but no information has yet been obtained on the magnitude of the difference of the quality between first excellent and just acceptable prints, nor has any information been obtained from which conclusions can be drawn as to the relation of the quality of the other prints in the series to the two specific qualities exhibited by the just acceptable and first excellent prints. It now seems desirable to obtain if possible some general relationship between negative exposure and reproduction quality. Here, again, there are two possible methods of approach, one being statistical, depending on the average opinion of a large group of observers, and the other, the sensitometric method, in which various sensitometric characteristics are combined in such a manner as to yield a number indicative of the reproduction quality. The latter method is of course based on the theory of tone reproduction.

Statistical Determination of Relative Print Quality.—It will be recalled that there are available four groups of prints made from four different negative materials. There are twelve prints in each group made from twelve negatives for which the exposures have been increased progressively from

the first to the last in the series. Each print is the best that can be made from its negative by using any of the available contrast grades of printing paper.

As the first step in applying the statistical method, the observer was instructed to arrange the prints in each group in an ascending order of quality, placing the poorest print at the extreme left and the best at the extreme right. In the group as finally arranged, therefore, there is, in the observer's opinion, a progressive increase in quality from print to print through the series from left to right. The positions in the series are indicated arbitrarily by numbers from 1 to 12, 1 being assigned to the worst quality and 12 to the best.

This judgment was carried out by thirty observers, twenty of whom made two independent series of judgments separated by a sufficient interval of time so that on the second occasion they could have no accurate memory of what they had done in the first judging. As typical of the results obtained by this method, the data in Table X are shown. Observers 1 and 2

TABLE X.

Typical Data. Judgment of Relative Print Quality

Observer	Position in Series											
	1	2	3	4	5	6	7	8	9	10	11	12
	Print Number											
1	1	2	3	4	5	6	7	11	8	9	12	10
	1	2	3	4	5	6	7	8	11	9	10	12
	1	2	3	4	5	6	7	11	9	8	12	10
2	1	2	3	4	5	6	7	10	9	12	8	11
	1	2	3	4	5	6	10	9	7	12	11	8
3	1	2	3	4	5	6	7	11	8	9	12	10
	1	2	3	4	5	6	7	11	12	10	9	8
4	1	2	3	4	5	6	7	11	12	10	9	8
	1	2	3	4	5	6	7	12	11	8	9	10

may be classed as experts in the judgment of photographic quality, having had many years of experience in this field. Observer 3 may be classed as an amateur, while 4 had had considerable experience in the judgment of photographic

quality but not sufficient to entitle him to be considered as an expert. It will be seen that these observers (and the same is true, in general, of the entire thirty observers) agreed perfectly on the relative quality of the first six prints. Agreement was almost unanimous on the seventh print, while the opinion on the relative quality of prints 8 and 12, inclusive, varied enormously among these observers and, in fact, an observer seldom, if ever, arranged the last five prints in exactly the same way. It will be noted that there is a marked disagreement, even between the two experts, as to the position print No. 8 should occupy in the series, observer No. 2 giving it on one occasion first place, and on the second occasion, second place, and the other expert placing it once each in the 8th, 9th, and 10th positions.

Having obtained fifty judgments on each of the four sets of prints, it is necessary to combine these values in some way to give numbers indicative of relative quality. There are several possible methods of summarizing data of this kind. The one used was to consider that, if a given print were placed in position 12, a score of 12 points should be assigned for that judgment, while if it were placed in position 8, a score of 8 points should be assigned. The method of scoring the prints is illustrated in Table XI for prints No. 12 and No. 11 made

TABLE XI.

Illustration of Method of Evaluating Relative Quality Judgments

Score for Print No. 12			*Score for Print No. 11*		
No. of Times	Position	Points	No. of Times	Position	Points
8	12	96	9	12	108
13	11	143	13	11	143
12	10	120	3	10	30
13	9	117	7	9	63
2	8	16	17	8	136
2	7	14	1	7	7
Total 50		506	Total 50		487

on material *C*. For instance, in the first column the numbers indicate the number of times print No. 12 was placed in the positions indicated by the numbers in the second column. Thus, print No. 12 was found in position 8 eight times, in

position 11 thirteen times, in position 10 twelve times, etc. By multiplying the numbers in columns one and two, those in the last column (points) are obtained, and by adding the numbers in the last column, the total score is obtained. Thus, on material *C*, print No. 12 had a score of 506, while No. 11 had a score of 487.

A summary of the score of the prints made from material *C* is shown in Table XII. In the line designated as "Actual"

TABLE XII.

Summary of the Score of the Prints Made from Material C.

Final Score	Print No.											
	1	2	3	4	5	6	7	8	9	10	11	12
Actual.....	50	100	150	200	272	305	393	479	481	477	487	506
Normal.....	50	100	150	200	250	300	350	400	450	500	550	600
Adjusted.....	10	21	31	41	56	63	81	98	99	98	100	104

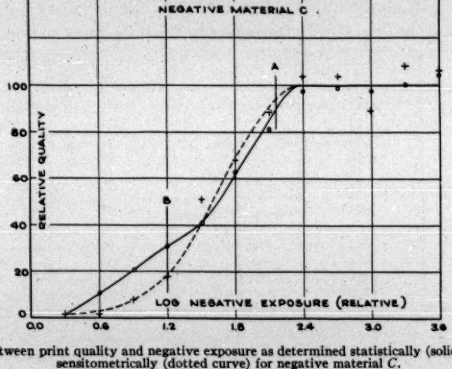
are the total scores for each print as obtained by the method illustrated in Table XI. In the line immediately below, designated as "Normal," are the results that would have been obtained had the prints been placed by all observers in their normal position according to negatives Nos. 1, 2, 3, etc. to 12. It will be observed that the first four prints received what may be called normal scores, indicating that all observers agreed that there was a progressive increase in quality as negative exposure was increased. Print No. 5 obtained a score slightly above its normal value. The same is true of prints Nos. 6, 7, 8, 9, and 10, while 11 and 12 obtained scores below the normal value. The fact that the scores for prints 8 to 12, inclusive, are so nearly equal may be interpreted as indicating that there is very little difference in the quality of the prints in this group as based upon the judgment of the entire group of observers. In fact, the differences of quality are so small that the positioning of the prints within this group is almost one of probability, which would, of course, result in identical scores for all prints within that group.

Adjusted values of relative print quality, as shown in the last line of the table, were obtained by taking the average score of prints 8 to 12, inclusive, as 100, and adjusting all of

the actual scores proportionately. These adjusted values are shown graphically in Fig. 36. The small circles represent the plotted values and the solid curve is drawn in what appears to be its most probable position.

Reference to previous judgments of the prints made on this material shows that the first excellent print, as based on the judgment of two hundred observers, came from negative No. 7.2. The vertical line *A* represents the position of this negative on the negative exposure scale, and it will be seen that a print made from this negative should have, on the

FIG. 36.



Relation between print quality and negative exposure as determined statistically (solid curve) and sensitometrically (dotted curve) for negative material *C*.

basis of the *arbitrary* print quality scale, a value of 86 per cent. The vertical line *B* shows the position of the previously determined just acceptable print on the arbitrary quality scale.

It must be remembered that the arbitrary scale of relative quality has no absolute significance, nor is it necessarily linear with reproduction quality. The fact that a straight line is obtained for the first four prints must not be interpreted to mean that the difference in reproduction quality between prints 1 and 2 is the same as that between 2 and 3 or 3 and 4. This line is straight simply by virtue of the fact that consecutive numbers 1, 2, 3, etc., were assigned to the

positions in which the prints were placed when being arranged in order of reproduction quality. It is quite possible to obtain direct experiential judgment of the magnitude of quality differences between adjacent prints. Such judgments, however, are difficult to make, especially by observers unskilled in this type of psychophysical observation. However, some data of this type have been accumulated, but many more observations are needed before it can be said with confidence that the *shape* of the reproduction quality-negative exposure curve has been established with certainty.

In Table XIII are shown the final adjusted values of relative quality for the four negative materials, and these

TABLE XIII.

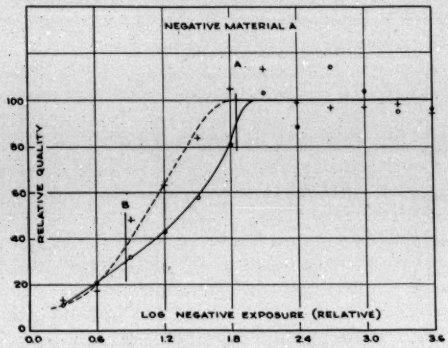
Relative Print Quality Determined by Direct Judgment of Prints

No.	*A*	*B*	*C*	*D*
1	11	11	10	10
2	21	22	21	20
3	32	33	31	31
4	42	45	41	41
5	58	56	56	51
6	81	84	63	62
7	103	104	81	74
8	88	84	98	95
9	114	114	99	104
10	104	102	98	91
11	95	102	100	100
12	96	95	104	110

are shown graphically in Figs. 36 to 39, inclusive. The vertical lines designated as *A* in all cases represent the position of the first excellent print and it will be seen that such prints have relatively high reproduction quality, in all cases greater than 86. It must be emphasized again that the scale of relative quality is a distorted one and the results as plotted from the data treated in the manner chosen may appear somewhat misleading unless this fact is kept constantly in mind. Thus, in the case of material *A*, Fig. 37, it appears that the difference in quality between prints 7 and 8 or between 8 and 9 is relatively great, in fact much greater than the difference in quality between prints 1 and 2 or 4 and 5. This, however, is not the case, as is evidenced by the fact that the

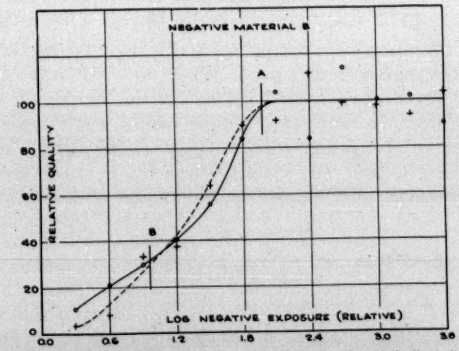
observers disagreed enormously among themselves as to the position of prints 7, 8, 9, and 10 in the series of ascending

FIG. 37.



Relation between print quality and negative exposure as determined statistically (solid curve) and sensitometrically (dotted curve) for negative material *A*.

FIG. 38.

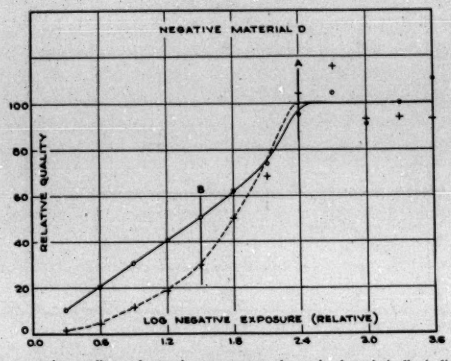


Relation between print quality and negative exposure as determined statistically (solid curve) and sensitometrically (dotted curve) for negative material *B*.

qualities, while there was absolutely no disagreement among the observers as to the positions of prints 1, 2, 3, 4, and 5 in the series of ascending qualities. The observers were un-

animous in agreeing that print 2 showed better quality than print 1, that print 3 showed better quality than print 2, etc. But for prints in the group from 7 to 12, inclusive, the disagreement was great, proving that the magnitude of the

FIG. 39.



Relation between print quality and negative exposure as determined statistically (solid curve) and sensitometrically (dotted curve) for negative material *D*.

quality differences between prints in this region is *much smaller* than in the region from 1 to 6.

Some preliminary observations on the *magnitude* of the quality difference between adjacent prints when arranged in the order of ascending quality (see Table XIV) indicate quite strongly that the quality interval between prints is relatively small but definite in the region of least negative

TABLE XIV.

Relative Position of Each Print in the Ascending Quality Series Determined by the Average of 30 Observers

	Judgment Positions											
	1	2	3	4	5	6	7	8	9	10	11	12
A.....	1	2	3	4	5	6	8	11	12	7	10	9
B.....	1	2	3	4	5	6	8	12	10	11	7	9
C.....	1	2	3	4	5	6	7	10	8	9	11	12
D.....	1	2	3	4	5	6	7	10	8	11	9	12

exposure. This quality interval then increases markedly as negative exposure is increased and becomes much smaller, practically zero, in the region of high negative exposures. Hence, if a quality scale is constructed in which equal units represent equal quality differences, the shape of these relative quality curves shown in Figs. 36 to 39, inclusive, will be considerably modified and the relatively large departures of some of the values in the high exposure region will be considerably reduced.

This situation is illustrated in a slightly different way in Table XIV. In this case the numbers at the top of the table represent consecutive positions in the quality series, while the numbers in the body of the table apply to the print numbers or negative numbers based upon the average of fifty judgments. Thus, for material *A* the No. 9 print was considered to be the best of the twelve, while No. 10 won second place, No. 7, third place, No. 12, fourth place, No. 11, fifth place, and No. 8, sixth place. The remainder were placed in the same positions as indicated by the negative or print, No. 1 being the worst, No. 2, second worst, etc.

It is felt that the qualitative relation between photographic quality and negative exposure has been established with considerable certainty. However, a quantitative evaluation of this relationship has not yet been obtained. The curves in Figs. 36 to 39, inclusive, indicate very definitely the minimum negative exposure which results in approximately maximum obtainable reproduction quality and, if the *effective camera speeds* of negative materials are defined in terms of this criterion, it is now possible to assign speed numbers meeting the requirements of the definition.

Sensitometric Determination of Relative Print Quality.—It is, of course, extremely desirable to be able to compute reproduction quality from direct sensitometric measurements. A determination of this aspect of photographic materials by the statistical methods discussed in the previous section is very laborious and quite prohibitive where it is desired to obtain information on a large number of photographic materials. The possibility of computing from sensitometric measurements numbers that will be indicative of reproduction quality will now be considered.

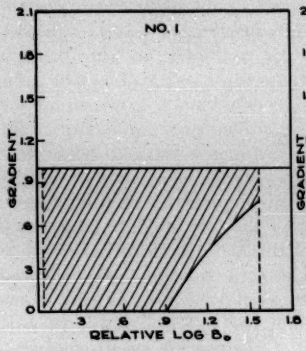
The position has been taken previously in this discussion that the most important characteristic of a photographic reproduction technic is the satisfactory reproduction of *brightness differences*, and it has been pointed out that the ability of a photographic material to fulfill this prime requisite is most significantly indicated in terms of the *gradient* of the density-log E curves of the negative and positive materials used. It seems logical, therefore, in seeking a characteristic of the reproduction which will be a measure of the photographic quality, an indication of the perfection with which the primary function of the photographic process has been accomplished, to turn first to a consideration of the gradient characteristics of the reproduction. Attention has already been called to the fact that the gradient errors may be expressed graphically as illustrated in Fig. 3. Here were plotted the values of gradient as a function of the log brightness of the object. If it be assumed that the best photographic quality is obtained when the contrast in the object is reproduced perfectly, it is obvious that a horizontal line drawn through the point where gradient is equal to 1.00 will represent maximum quality. The difference between the gradient at any point on the reproduction curve and unity (1.0) is the gradient error, ΔG, for that particular object brightness, and it seems logical to expect that the over-all print quality may be determined in some way by an evaluation of the magnitudes of these gradient errors.

It must be recognized, however, that, under certain conditions, the brightness contrast in the object may be so great that it is quite impossible to hope to render the entire brightness scale of the object by a reproduction curve (see curve E in Fig. 2) which is a straight line at 45 degrees to the OX axis on which the object brightness scale is established. This is evident because, for the vast majority of commercially available developing out papers of glossy surface, the density scale is only approximately 1.7 (corresponding to a brightness contrast of 50), and in many cases brightness contrast in objects to be photographed may go to values considerably in excess of 50. Under such circumstances a compression of the object brightness scale is necessary, and it is quite possible that the best reproduction quality may be represented

by a straight line having a slope somewhat less than 45 degrees. In such cases it may be necessary to use some value less than unity in terms of which to compute gradient errors used in the evaluation of print quality. For instance, if a log brightness range of 1.7 be assumed to be available in the positive material, and if the log brightness scale of the object be 2.12, a straight line reproduction curve having a gradient of 0.8 rather than 1.0 will give perfect *proportional* rendering of the contrast throughout the entire brightness scale of the object being reproduced. Under such conditions it may be found that, in order to evaluate gradient errors in such a way as to yield a measure of print quality, they should be determined as differences from 0.8 rather than 1.0. On the other hand, it does not seem likely that such a course should be followed generally. In case the brightness contrast in the object exceeds the brightness scale which the eye is capable of perceiving under the particular set of visual field conditions being considered, it is obviously quite unnecessary to attempt to record in the reproduction the entire range of object brightnesses. Even if they could be reproduced they would not be perceived by the eye. It seems almost certain that in arriving at a final conclusion of how the gradient errors should be computed most significantly for the evaluation of reproduction quality, a limit to the contrast compression is set by the characteristics of the visual mechanism which determine the ratio of the brightness of *subjective white* to *subjective black*. Moreover, when objects of low contrast are considered, a further modification of the method of evaluating the effective gradient may be necessary.

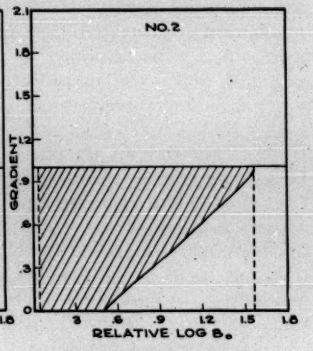
For the purpose of this discussion, attention will be confined to the development of a method for evaluating print quality in terms of gradient for the particular object used (Fig. 4), which, with the camera and lens system employed in making the original negative, gave an image illumination contrast of 28 ($\log IS_i = 1.45$). In making the test object transparency this contrast was increased (proportionately throughout the scale) to 32 ($\log IS_i = 1.50$), which is considered *average* image illumination contrast. It will be shown in the following pages that, by proper evaluation of gradient and gradient errors, numbers can be obtained which are satisfactory indices of over-all print quality.

FIG. 40.



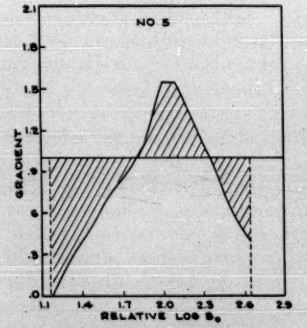
Gradient error curve for the best reproduction made with negative No. 1.

FIG. 41.



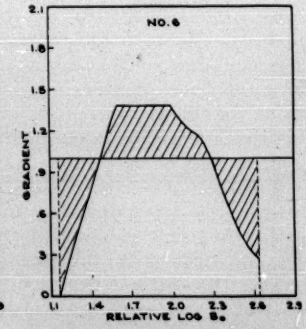
Gradient error curve for the best reproduction made with negative No. 2.

FIG. 44.



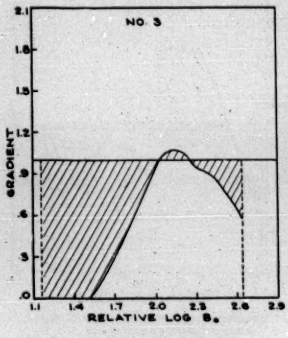
Gradient error curve for the best reproduction made with negative No. 5.

FIG. 45.



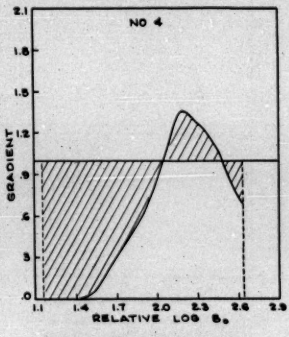
Gradient error curve for the best reproduction made with negative No. 6.

FIG. 42.



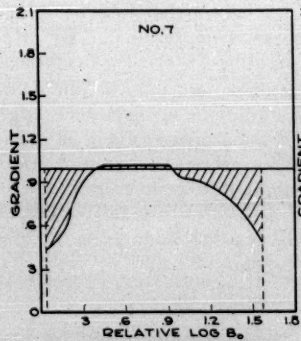
Gradient error curve for the best reproduction made with negative No. 3.

FIG. 43.



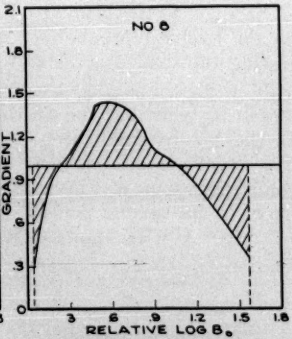
Gradient error curve for the best reproduction made with negative No. 4.

FIG. 46.



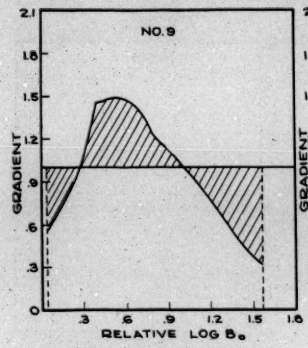
Gradient error curve for the best reproduction made with negative No. 7.

FIG. 47.



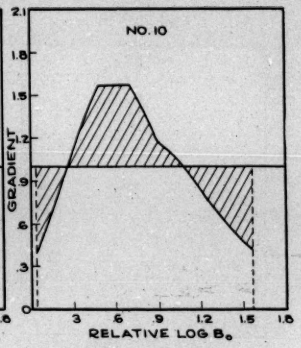
Gradient error curve for the best reproduction made with negative No. 8.

FIG. 48.



Gradient error curve for the best reproduction made with negative No. 9.

FIG. 49.



Gradient error curve for the best reproduction made with negative No. 10.

The relationship which is to be evaluated may be expressed formally as:

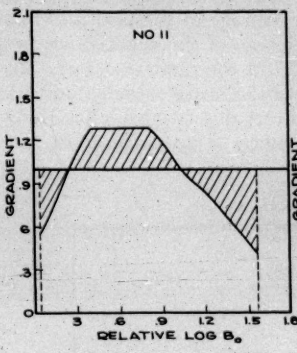
$$\text{Reproduction quality, } Q = f(G).$$

In Figs. 40 to 51, inclusive, are shown the gradient curves for the twelve reproductions made on material *C*. By integrating the shaded areas and dividing by the log brightness scale, values of average gradient error, $\overline{\Delta G}$, may be obtained. It will be seen that the average gradient error is very large for the No. 1 reproduction, decreasing progressively as negative exposure is increased. It is obvious, therefore, that quality is in some way inversely related to the average gradient error; in the second column of Table XV are the values of $1 - \overline{\Delta G}$. A first approximation to the evaluation of the relationship already suggested becomes:

$$Q = 1 - \overline{\Delta G}.$$

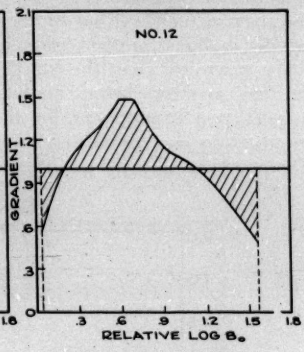
In such a formulation if $\overline{\Delta G}$ becomes unity, then quality becomes zero; while, if the reproduction curve becomes coincident with a horizontal line drawn through the point where gradient is equal to 1.0, $\overline{\Delta G}$ becomes 0 and $1 - \overline{\Delta G}$ becomes 1. Thus, a scale extending from zero to unity is obtained, on which relative print quality may be expressed in this form.

FIG. 50.



Gradient error curve for the best reproduction made with negative No. 11.

FIG. 51.



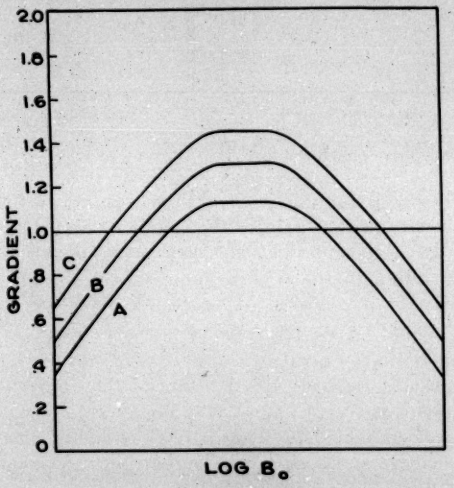
Gradient error curve for the best reproduction made with negative No. 12.

An inspection of the gradient curves shown in Figs. 40 to 51, inclusive, and of many other similar curves shows that frequently two reproductions may have equal values of $1 - \overline{\Delta G}$, although the corresponding prints display marked differences in excellency. It appears, therefore, that the $1 - \overline{\Delta G}$ value is not entirely adequate to express quality. It is found that equivalent values of $\overline{\Delta G}$ may be associated with markedly different *distributions* of the errors along the brightness scale, and, moreover, equal values of $1 - \overline{\Delta G}$ may be accompanied by wide variations in the relation between negative and positive gradient errors. A consideration of this subject from a purely theoretical standpoint seems to indicate that it should be reasonable to assume that, if a certain magnitude of average gradient error must be tolerated, this lack of perfection in the reproduction of contrast will be least injurious to the reproduction quality when these gradient errors are properly balanced between negative and positive values and also, probably, between the highlight and shadow regions.

This is illustrated in Fig. 52. The three gradient reproductions represented by the gradient curves have the same value of $\overline{\Delta G}$. In the case of A, the gradient errors are pre-

dominantly negative. In the case of B, the average of the negative errors is equal to the average of the positive errors, while, in the case of C, the average of the negative errors is very much less than the average of the positive errors. It is to be expected from theoretical considerations, and this expectation is supported by direct observation, that the reproduction as represented by curve B gives the most satisfactory over-all print quality.

FIG. 52.



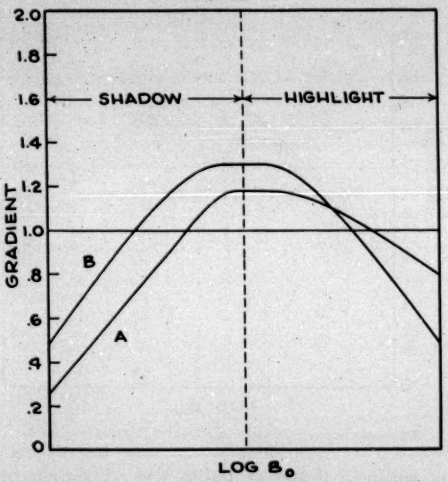
Illustrating different positive-negative distributions of gradient errors.

Figure 53 illustrates the effect of highlight and shadow distribution of gradient errors. The highlight and shadow regions are separated arbitrarily by a vertical line drawn at a point midway between (on the logarithmic scale) the maximum and minimum object brightnesses. In the case of reproduction A, the average of the errors in the shadow region is considerably greater than the average of the errors in the highlight region, while, in the case of reproduction B, a fairly close equality between the average shadow gradient errors and

the average highlight gradient errors is obtained, and here, again, the print quality which is obtained when shadow and highlight gradient errors are balanced is appreciably higher than when they are not.

It seems necessary, therefore, to apply certain corrective coefficients to the expression $1 - \overline{\Delta G}$ which will compensate for the various possible *distributions* of the gradient errors. One method of obtaining the values for the corrective coeffi-

FIG. 53.



Illustrating different shadow-highlight distributions of gradient errors.

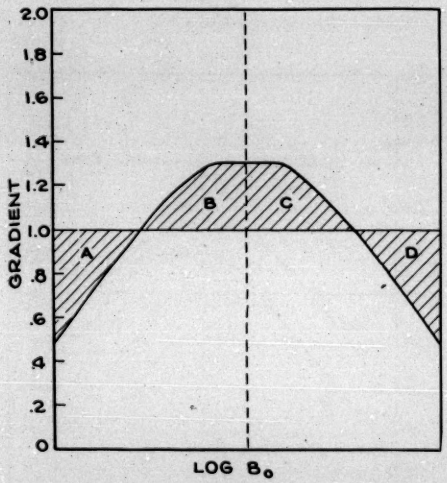
cients is illustrated in Fig. 54. The letters A, B, C, and D represent the areas of the shaded portions in which they are located. The positive-negative balance will be indicated by k_1, and the highlight-shadow balance by k_2. These coefficients are defined by the following equations:

$$k_1 = 1 - \frac{(B + C) - (A + D)}{BS_0},$$

$$k_2 = 1 - \frac{(A + B) - (C + D)}{BS_0}.$$

In practice, the value of these corrective factors can be determined very conveniently by mechanical integration from the original graph sheets. In each case the final value can be determined by a single planimeter operation by proper manipulation of the planimeter.

FIG. 54.



Illustrating method of computing corrective coefficients k_1 and k_2.

In the second column of Table XV are shown the values of $1 - \overline{\Delta G}$ for each of the twelve prints made on negative material *C*. These start from a relatively low value and increase progressively to an approximately constant value for the last six prints in the group. It is quite obvious from a consideration of these values that they are not satisfactory indices of reproduction quality. For instance, reproduction No. 3, for which $1 - \overline{\Delta G}$ is 0.54, has extremely poor quality, while reproductions from 8 to 12, inclusive, which are of excellent quality, have values of $1 - \overline{\Delta G}$ of approximately 0.70. In the third and fourth columns of the table are shown

the values of the corrective factors k_1 and k_2, as previously defined, and in the column Z_1 are shown the values obtained by applying these corrective coefficients to $1 - \overline{\Delta G}$ in each case.

In the column Q_0 are the values obtained by the direct judgment of print quality (see Table XIII). These values have been adjusted so that the average of the prints 8 to 12, inclusive, is 100. In order to compare conveniently the values of computed quality with those of observed quality, values of Z_1 have also been adjusted so that the average for prints 8 to 12, inclusive, is 100, as shown in column Z_2. It

TABLE XV.

Values of Quality Computed from Characteristics of the Reproduction Curve
Negative Material C

No.	$1-\overline{\Delta G}$	k_1	k_2	Z_1	Z_2	Q_0	k_3	Z_3	Z_4
1	0.18	0.18	0.65	0.03	5	10	0.17	1	1
2	.33	.33	.48	.05	8	21	.35	3	3
3	.54	.56	.30	.09	14	31	.53	8	8
4	.53	.66	.52	.18	27	41	.64	17	18
5	.62	.85	.95	.48	73	56	.80	58	61
6	.63	.96	.96	.53	80	63	.83	64	68
7	.85	.86	.99	.72	109	81	.78	85	89
8	.74	.99	.94	.68	103	98	.96	99	104
9	.69	.99	.94	.64	97	99	.91	89	93
10	.68	.94	.86	.55	84	98	.97	82	89
11	.76	.99	.99	.74	112	100	.92	103	108
12	.77	.93	.92	.69	104	104	.97	101	106
Average of best five				.66	100	100		94	100

will be seen that the agreement between computed and observed quality is now much better than when computed quality was based on $1 - \overline{\Delta G}$ without the corrective coefficients which take care of unbalance of gradient errors between negative and positive values and between the highlight and shadow regions.

It is evident, however, that the agreement between observed and computed quality (Q_0 and Z_2) is still not very satisfactory. The disagreement is greatest in the region of medium negative exposure. For instance, the computed values for prints 5, 6, and 7 are considerably greater than those assigned to these prints by direct observation. It seems necessary, therefore, to seek further for some additional

factor that will serve to correct this undesirable disagreement. Again, it seems most logical to expect to find this in some aspect of the gradient of the reproduction curve. In the second section of Table XVI are given certain characteristics of the reproductions derived sensitometrically. The column headings have the following significance:

G_s is the gradient at the extreme shadow end of the tone reproduction curve.

G_h is the gradient at the extreme highlight end of the tone reproduction curve.

G_{max} is the maximum gradient of the tone reproduction curve, and if straight-line portions of both negative and positive materials are effective in producing this maximum gradient, G_{max} becomes the gamma of the reproduction curve and is equivalent to the product of the negative and positive gammas.

$\bar{G}_r$ is the average gradient of the tone reproduction curve and is, of course, equal to the density scale of the reproduction divided by the log brightness scale of the test object.

DS_r is the density scale of the reproduction.

In the first section of Table XVI are shown the values of Z_2 and Q_0 taken from Table XV. The computed quality

TABLE XVI.

Comparison of Computed and Observed Quality Values with Various Specific Sensitometric Characteristics of the Reproduction

Photographic Material C

No.	Quality			Reproduction Characteristics				
	Z_2	Q_0	Z_4	G_s	G_h	G_{max}	$\bar{G}_r$	DS_r
1	5	10	1	0.0	0.74	0.73	0.18	0.28
2	8	21	3	.0	.92	.90	.25	.38
3	14	31	8	.00	.58	1.06	.55	.84
4	27	41	18	.00	.68	1.35	.68	1.04
5	73	56	61	.00	.40	1.55	.85	1.30
6	80	63	68	.10	.29	1.38	.92	1.40
7	109	81	89	.46	.46	1.02	.82	1.26
8	103	98	104	.49	.39	1.43	1.02	1.55
9	97	99	93	.60	.35	1.48	.96	1.47
10	84	98	89	.51	.45	1.57	1.05	1.60
11	112	100	108	.61	.44	1.29	.98	1.49
12	104	104	106	.64	.55	1.48	1.03	1.58
Av.	100	100	100				1.01	1 54

value (Z_2) of reproduction No. 7 is 109, which is slightly above the average of the five excellent prints (8 to 12), but, according to the direct observational data, this reproduction is appreciably lower in quality than the average of the five excellent prints. The average gradient error for print No. 7 was very low and the distribution of the errors was well balanced. This accounts for the high value of computed quality. However, an inspection of the second section of the table shows that the average gradient of this reproduction was only 0.82 as compared with an average of 1.01 for the five excellent reproductions. Moreover, the density scale of reproduction No. 7 was only 1.26, while the average density scale of the five excellent reproductions was 1.54.

It appears necessary, therefore, for a reproduction to be adjudged of good quality by the direct experiential estimate, that the average reproduction gradient (for this particular object at least) should be of the order of 1.0 or that the density scale should approach more closely to the log brightness scale of the object, or both. Incidentally the average density scale of the five excellent prints in this group is approaching quite closely to the useful density scale of the printing papers on which these prints were made. Thus, the observers in judging the quality of this group of prints demand either the use of approximately the entire useful density scale of the positive material or an average gradient of approximately 1.0. It is rather difficult to decide which of these is the critical requirement. It does not seem necessary, however, to make a decision as to which of the two factors is basically predominant because, in fact, they are not independent variables.

Hence, it seems desirable to evaluate a third corrective coefficient based upon average gradient and density scale of the reproduction. If the rôle of density scale in contributing to high reproduction quality is considered first, it seems logical that the percentage of the unused density scale of the positive material constitutes an index of the loss in reproduction quality, thus leading to a formulation of a corrective factor as follows:

$$k_3 = 1 - \left(\frac{DS_v - DS_r}{DS_v} \right),$$

in which DS_y is the density scale of the positive material on which the print is made and DS_r is the density scale of the reproduction. This reduces to

$$k_3 = \frac{DS_r}{DS_y} = \frac{DS_r}{BS_0} \cdot \frac{BS_0}{DS_y} = \bar{G}_r \cdot \frac{BS_0}{DS_y}.$$

This corrective coefficient contains implicitly both average gradient and density scale factors.

While the reproductions made on material *C* show a maximum density scale, DS_r, of 1.60, those made using some of the other photographic negative materials rise somewhat higher and an inspection of the characteristic curves of the positive materials indicates that a value of 1.65 may be taken to represent the maximum useful density scale of these positive materials *as a group*. The third corrective coefficient, k_3, will be formulated on this basis using, of course, the value of 1.51 for the log brightness scale of the test object:

$$k_3 = \bar{G}_r \cdot \frac{1.51}{1.65} = 0.91 \cdot \bar{G}_r.$$

Returning to a consideration of Table XV, under k_3 will be found the value of the third corrective coefficient computed as outlined above. In the column Z_3 are the products of k_3 by Z_2, and in the last column, Z_4, these values are adjusted so as to give an average of 100 for the prints 8 to 12, inclusive. It will be seen that this has considerably reduced the discrepancy between the observed Q_0 and the values computed from sensitometric data, using only k_1 and k_2 as corrective factors. These results are shown graphically in Fig. 38. The points represented by small circles are those of Q_0, and the solid curve is drawn in its most probable position to fit these points. As stated before, the fact that there is such an enormous disagreement among the thirty observers on the relative quality of prints 8 to 12, inclusive, has lead to the conclusion that actually there is very little difference in the qualities of the five prints, and therefore the use of a straight line at 100 to represent the quality of these five prints seems justified. The points represented by crosses are values of Z_4, and the dotted line indicates the most probable position of the quality curve based upon computed values.

It will be seen that the computed values for the first four prints in the series are much lower than the observed values, but, as pointed out, the mode of treating the judgment values is such as to give no information on the absolute magnitude of the quality differences between prints in this region. A few direct observations on the relative magnitude of the quality differences between adjacent prints leads to the conviction that the dotted curve based upon sensitometric computation agrees much better with the actual experiential evaluation of these quality differences.

Table XVII gives quality and reproduction characteristic values for photographic material *A*. Here, again, it is found

TABLE XVII.

Comparison of Computed and Observed Quality Values with Various Specific Sensitometric Characteristics of the Reproduction
Photographic Material A

No.	Quality			Reproduction Characteristics				
	Z_2	Q_0	Z_4	G_s	G_h	G_{max}	$\overline{G}_r$	DS_r
1	25	11	13	0.0	0.92	1.49	0.54	0.83
2	26	21	15	.00	.50	1.50	.56	.85
3	65	32	50	.00	.31	2.40	.83	1.27
4	91	42	64	.00	.50	1.7	.76	1.17
5	104	58	84	.19	.44	1.36	.87	1.33
6	123	81	105	.32	.51	1.26	.92	1.41
7	117	103	114	.49	.61	1.57	1.01	1.55
8	112	88	99	.73	.35	1.66	1.00	1.53
9	91	114	97	.48	.37	1.72	1.09	1.66
10	93	104	97	.53	.50	1.79	1.07	1.63
11	96	95	98	.43	.50	1.49	1.05	1.61
12	91	96	95	.40	.54	1.58	1.07	1.63
Av.	100	100	100				1.05	1.60

that the values of computed quality, Z_2, involving corrective coefficients k_1 and k_2 give unduly high values to reproductions in the region of prints Nos. 4, 5, and 6, and inspection of the average gradient and density scale values for these reproductions shows again that they are appreciably lower than the average of the same values for the six excellent prints of this group. Again, the average gradient errors are relatively low for these prints, and the unbalance between positive and negative and highlight and shadow distribution is not very

serious. The reproductions, however, are judged as lacking in excellence because of low values of average gradient and density scale. By applying the corrective coefficient k_3, the values Z_4 are obtained and again the agreement between computed and observed values is improved but not to the same extent as in the case of material *C*. Prints 4, 5, and 6 seem to have too high values to correspond with observed evaluations. These data are shown graphically in Fig. 37.

Similar data for the photographic materials *B* and *D* are shown in Tables XVIII and XIX, and graphically in Figs. 38

TABLE XVIII.

Comparison of Computed and Observed Quality Values with Various Specific Sensitometric Characteristics of the Reproduction

Photographic Material B

No.	Quality			Reproduction Characteristics				
	Z_3	Q_0	Z_4	G_s	G_h	G_{max}	$\bar{G}_r$	DS_r
1	12	11	4	0.00	0.65	1.53	0.36	0.55
2	18	22	8	.00	.36	.88	.41	.63
3	51	33	35	.00	.27	1.29	.70	1.07
4	57	45	38	.00	.48	1.07	.65	1.00
5	78	56	64	.19	.39	1.41	.82	1.25
6	104	84	90	.54	.49	1.16	.86	1.31
7	94	103	92	.38	.43	1.67	.95	1.46
8	114	84	113	.37	.65	1.30	.93	1.42
9	96	114	99	.56	.46	1.62	1.03	1.57
10	96	102	98	.44	.38	1.46	1.00	1.53
11	94	102	94	.59	.53	1.81	.99	1.51
12	106	95	104	.80	.40	1.48	.98	1.49
Av.	100	100	100				.98	1.50

and 39. These support strongly the arguments already advanced relative to the sensitometric characteristics of photographic reproductions which are considered as good quality. In the case of *B*, the six prints 7 to 12, inclusive, are of approximately equal quality and, as noted before, the $\bar{G}_r$ and the DS_r values are also fairly constant and relatively high. A drop in these values seems to produce immediate loss in quality as judged directly by the group of observers. In the case of material *B*, the application of the k_3 coefficient tends to produce quite good agreement between the computed and observed values. In the case of material *D*, the agree-

ment in the region of low negative exposure is not particularly good, but, as stated before, the computed scale is probably more nearly in accord with the actual values of quality differences than those based on the arbitrary scale in terms of which observed quality is expressed. It is known quite definitely, from direct experiential evaluation, that the quality difference, let us say, between prints 1 and 2, or 3 and 4, is considerably greater than the difference between any pair of prints in the group 8 to 12, inclusive. This is not indicated by the scale as established.

TABLE XIX.

Comparison of Computed and Observed Quality Values with Various Specific Sensitometric Characteristics of the Reproduction
Photographic Material D

No.	Quality			Reproduction Characteristics				
	Z_2	Q_0	Z_4	G_s	G_h	G_{max}	$\bar{G}_r$	DS_r
1	7	10	2	0.0	0.82	0.95	0.23	0.35
2	14	20	5	.0	.76	.86	.36	.55
3	23	31	12	.0	.68	.98	.50	.76
4	36	41	20	.0	.40	1.13	.58	.88
5	42	51	30	.0	.14	1.51	.72	1.10
6	61	62	50	.10	.08	1.75	.84	1.28
7	77	74	69	.12	.42	1.32	.78	1.20
8	116	95	104	.48	.38	1.34	.91	1.39
9	117	104	116	.68	.48	1.35	1.01	1.55
10	91	91	93	.48	.48	1.95	1.03	1.58
11	88	100	94	.80	.56	1.95	1.08	1.65
12	88	110	93	.92	.53	1.78	1.07	1.63
Av.	100	100	100				1.02	1.54

In Figs. 36 to 39, inclusive, the vertical lines designated as *A* represent the negative exposure (or negative number) that gives a negative from which the first excellent print was made. It will be seen in all cases that this is located very close to the beginning of the horizontal straight portion of the quality curve representing maximum quality. In fact, the quality value, based either on computed or on observed data, differs from the average of the five or six prints of maximum quality by less than differences within these groups themselves. The vertical lines designated as *B* represent the negative exposure (or negative number) giving a negative

from which a just acceptable print could be made. It is obvious that the quality of these prints is very low as compared with the best prints.

Attention is drawn to one other characteristic of the reproductions which show excellent quality. These consist of reproductions Nos. 7 to 12, inclusive, in the case of materials *A* and *B*, and of 8 to 12, inclusive, in the case of materials *C* and *D*. The values referred to are those of gradient in the extreme shadow and extreme highlight regions shown in columns G_s and G_h. For very low values of negative exposure the extreme shadow gradient is zero, while the extreme highlight gradient is high. As negative exposure is increased, the shadow gradient increases while the highlight gradient tends to decrease. When the negative exposure has exceeded the point where a print of excellent quality can be produced therefrom, the shadow and highlight gradients tend to be approximately equal, on the average, the shadow gradient being somewhat higher than the highlight gradient. It is thought that the excellence of print quality is influenced by this relationship. It appears reasonable to suppose, at least in a scene where the interest is distributed fairly well over the entire composition rather than concentrated in some particular part of the tonal scale, that about the same compression of detail will be tolerated in both the shadow and the highlight regions. If there is any departure from this equality, it should be towards a higher gradient in the extreme shadow than in the extreme highlight end. This may be accounted for and justified by the fact that, under ordinary conditions, prints are probably viewed under illumination such that the shadow regions are represented by print brightnesses which fall on the contrast sensitivity curve of the human eye below the straight-line portion, while the highlight areas fall on the straight-line portion of the curve. To produce a sensation (or perception) of equal detail reproduction, then, it is necessary to have somewhat greater objective brightness difference in the shadow region than in the highlight region.

XII. CONCLUSIONS.

It must be kept in mind that this paper has dealt entirely with a single object, one which is believed to have approxi-

mately average brightness contrast and uniformly distributed interest. It is not safe to assume, therefore, that the empirical formula derived here for computing reproduction quality will apply to the reproduction of other objects differing widely in character from the one used in this case. Also, as stated before, it would be unjustifiable to assume that the proposed sensitometric method of finding the minimum negative exposure which will result in the first excellent print will apply in the case of other scenes differing appreciably in composition from the one used (see footnote, page 513). It seems reasonable to conclude, however, that any formula which may eventually be found for the numerical expression of quality will be based largely upon various aspects of gradient, such as gradient errors and distribution, average gradient, limiting gradient, maximum gradient, etc. There is little doubt that the quality of a photographic reproduction is determined largely by the way in which *brightness differences* are reproduced.

Wirh regard to possible modes of expressing *effective camera speeds*, it seems certain that the most rational and logical solution of this problem lies in the adoption of a criterion defined in terms of *gradient*, not necessarily in terms of a gradient characteristic of the negative material alone, but by this factor evaluated in terms of the gradient characteristics of the positive material on which the print is made, and hence, ultimately, upon the gradient characteristics of the reproduction. It seems impossible to find in the theory of tone reproduction a justifiable reason for the use of *any* fixed value of *density* or the use of the *inertia* value as a criterion of *effective camera speeds*. Moreover, speed numbers to be of maximum utility, at least in the amateur field, should yield information on the *minimum* exposure which will result in a negative from which a *satisfactory* print can be made. While there may be some differences of opinion on the most rational definition of "satisfactory," as used above, the author is convinced that it is highly desirable to define as satisfactory a print of excellent quality approximating closely the maximum that can be obtained.

ACKNOWLEDGMENT

The experimental work on which this paper is based was done by a group of research workers during the last two or three years. To give full credit to each individual for his contributions would necessitate the publication of the material under the names of several co-authors. The author therefore owes to his coworkers a much greater obligation than can be paid by the usual acknowledgments of assistance. Mr. Marion Russell, who is in charge of the sensitometric section of the physics department of these laboratories, has contributed materially by planning and organizing the work and by giving advice and counsel in the interpretation of many of the results. Messrs. C. N. Nelson and H. R. Condit have carried out practically all of the experimental work, including the reduction of the data to the form in which it is presented. They are to be highly commended for the initiative and originality displayed in devising experimental technics yielding results of high precision, and for the diligence and great care exercised in conducting the work. The excellent agreement obtained by the interchecking of results is due largely to their efforts. The author wishes also to express his most hearty thanks to all those who so willingly gave their time and careful attention to the judging of print quality.

REFERENCES.

1. HURTER, F., AND DRIFFIELD, V. C.: "Photochemical Investigations and a New Method of Determination of the Sensitiveness of Photographic Plates," *Jour. Soc. Chem. Ind.*, **9**, 455, 1890.
2. SCHEINER, J.: "Ein Universal-Sensitometer," *Z. Instrumentenk.*, **14**, 201, 1894.
3. Publication No. 4512 of the Deutsche Industrie Normen, Jan. 1934. LUTHER R.: "Remarques sur la Méthode sensitométrique DIN," IXth Congrès Internat. de Photographie, Paris, July, 1935, p. 569.
4. JONES, L. A., AND RUSSELL, M. E.: "Minimum Useful Gradient as a Criterion of Photographic Speed," *Jour. Opt. Soc. Amer.*, **25**, 396, 1935.
5. SHEPPARD, S. E.: "Characteristics and Anomalies of Emulsions on Development," *Phot. J.*, **50**, 190, 1926.
6. LUTHER, R.: "Interpretation of the Characteristic Curve," *Phot. J.*, **49**, 185, 1925.
7. Bibliography on Tone Reproduction Theory:
   (*a*) HURTER, F., AND DRIFFIELD, V. C.: "Relation between Photographic Negatives and Their Positives," Jour. Soc. Chem. Ind., **10**, 100, 1891.

(*b*) RAYLEIGH, LORD: "On the General Problem of Photographic Reproduction, with Suggestions for Enhancing Gradation Originally Invisible," *Phil. Mag.*, **22**, 734, 1911.
(*c*) RENWICK, F. F.: "The Under-exposure Period in Theory and Practice," *Phot. J.*, **37**, 127, 1913.
(*d*) PORTER, A. W., AND SLADE, R. W.: "The Fundamental Law for the True Photographic Rendering of Contrast," *Phil. Mag.*, **38**, 187, 1919.
(*e*) RENWICK, F. F.: "The Fundamental Law for the True Photographic Rendering of Contrast," *Phil. Mag.*, **38**, 633, 1919.
(*f*) RENWICK, F. F.: "The Fundamental Law for the True Photographic Rendering of Contrasts," *Phil. Mag.*, **39**, 151, 1919.
(*g*) JONES, L. A.: "On the Theory of Tone Reproduction with a Graphic Method for the Solution of Problems," JOUR. FRANK. INST., **190**, 39, 1920.
(*h*) JONES, L. A.: "Photographic Reproduction of Tone," *Jour. Opt. Soc. Amer.*, **5**, 232, 1921.
(*i*) JONES, L. A.: "Photographic Rendering of Tone Values," JOUR. FRANK. INST., **189**, 469, 1920.
(*j*) GOLDBERG, E.: "The Formation of the Photographic Image," Encyclopedia of Photography, **99**, 1922.
(*k*) JONES, L. A.: "The Contrast of Photographic Printing Paper," JOUR. FRANK. INST., **202**, 177, 1926; **203**, 111, 1927; **204**, 41, 1927.
(*l*) MENDELSSOHN, T.: "Photographie als Tonwiedergabe," *Camera* (Luzern), **14**, 260, 1936. "Die Helligkeit des Mattscheibenbildes," *Camera* (Luzern), **13**, 294, 1935.
(*m*) HOPKINSON, R. G.: "The Photographic Representation of Street Lighting Installations," *Phot. J.*, **76**, 323, 1936. "A New Approach to the Problem of Tone Reproduction," *Phot. J.*, **77**, 542, 1937.
(*n*) RENWICK, F. F.: "Difficulties in Translating the Theory of Tone Reproduction into Practice and the Sensation of Contrast," *Phot. J.*, **77**, 6, 1937.
(*o*) ABRIBAT, M.: "Considérations générales sur le rendu photographique des luminosités" (3e Réunion de l'Institut d'Optique, April 9, 1935), reported in *Rev. d'Optique*, **14**, 40, 1935.
8. PITT, S. G. H., AND SELWYN, E. W. H.: "Color of Outdoor Photographic Subjects," *Phot. J.*, **78**, 115, 1938.
9. JONES, L. A.: "The Contrast of Photographic Printing Paper," J. FRANK. INST., **202**, 177, 1926; **203**, 111, 1927; **204**, 41, 1927.
10. ROMER, W.: "Sure l'Expression numérique de la rapidité des émulsions négatives," *Sci. Ind. Phot.*, (2), **7**, 117, 1936.
11. ROMER, W., MARKOCKI, W., AND BIERNAT, J.: "Expression numérique de la Sensibilité basée sur les Pentes minima relatives," IXth Congrès Internat. de Photographie, Paris, July, 1935, p. 605.
12. LUTHER, R.: "La Region de sous-exposition de la Courbe caractéristique," *Sci. Ind. Phot.*, **4**, 1, 1924.
13. LAPEYRE, F.: *Bull. soc. franç. phot.* (3), **16**, 10, 28 (1929).
14. (*a*) LABUSSIÈRE, G.: *Bull. soc. franç. phot.* (3), **10**, 45 (1933).
(*b*) LAPEYRE, F.: *Bull. soc. franç. phot.* (3), **16**, 10, 28 (1929); (3), **20**, 49 (1933).
(*c*) HESS AND AMÉDÉE-MANNHEIM, "La Photographie" (A. Colin, Paris, 1932), pp. 128–133.

(*d*) GOLDBERG, E.: "Der Aufbau des Photographischen Bildes" (W. Knapp. Halle, 1922).

(*e*) JONES, L. A., AND RUSSELL, M. E.: "The Expression of Plate Speed in terms of Minimum Useful Gradient," Proc. VIIth Internat. Congress Photography, July, 1928, p. 130.

(*f*) LUTHER, R.: "Remarques sur la Méthode sensitométrique DIN," IXth Congrès Internat. de Photographie, Paris, July 1935, p. 569.

(*g*) ROMER, W., MARKOCKI, W., AND BIERNAT, J.: "Expression numérique de la Sensibilité basée sur les Pentes minima relatives," IXth Congrès Internat. de Photographie, Paris, July, 1935, p. 605.

(*h*) DAVIS, R., AND NEELAND, G. K.: "Comparison of Several Developers and the Specification of Relative Sensitivity," *Bur. Stand. Jour. Res.*, 11, 379, 1933; *Sci. Ind. Phot.*, (2), 5, 12, 1934.

NOTES FROM THE NATIONAL BUREAU OF STANDARDS.*

RADIO SOUNDINGS AND WEATHER PREDICTION.

The past year has seen the transformation of the radio sonde (variously referred to as radio meteorograph or radio telemeter) from an experimental apparatus, requiring individual handling, into a commercial product, reasonably priced because of the application of mass production methods, and capable of meeting rigorous performance specifications under field operating conditions. Beginning June 1, 1938, the Navy Department inaugurated routine use of the radio sonde at the Naval Air Station, Washington, D. C., and during July 1938 the Weather Bureau began routine operation at Nashville, Tenn., Oklahoma City, Okla., Omaha, Neb., Oakland, Calif., Fargo, N. D., and Sault Ste. Marie, Michigan.

Radio sonde service at two other Navy Department stations and at two Army stations was started in December 1938. In all, during 1938, some 1,500 radio soundings were made in the regular course of collecting upper-air weather data.

The instrument used at the Weather Bureau and Navy Department stations is of the type developed at the National Bureau of Standards with funds provided by the Navy Department. The details of design and of the experimental performance of this device were published in several papers during 1937 and 1938. A paper by Harry Diamond, W. S. Hinman, Jr., A. H. Mears, and C. Harmantas, which will be published in the *Journal of Aeronautical Sciences* outlines the improvements incorporated in this radio sonde to facilitate mass production while retaining its accuracy of performance and simplicity of use. The engineering staff of Julien P. Friez & Sons Co. contributed materially to these improvements.

The paper also presents an analysis of the performance of the radio sonde in six months' routine use at seven aerological

* Communicated by the Director.

stations. A quantitative evaluation of the accuracy of the radio sonde observations under field operating conditions is included, and it is shown to be well within the specifications formulated by the Weather Bureau for this service. Upper-air observations of pressure, temperature, and humidity are obtained to an accuracy compatible with the requirements of the analytical methods employed in weather forecasting.

Based on Weather Bureau data, it is shown that the inauguration of radio sonde operation (in place of the airplane method of upper-air soundings hitherto employed) has resulted in a marked improvement in the regularity of the daily schedule of observations and in the altitude to which observations are made. To obtain full value from the upper-air observations, it is essential for the forecasting division to have complete and accurate information from all stations in its hands by 7:30 A.M. E.S.T., which is the time of the ground observations from which the morning daily maps are constructed. This requires that the actual upper-air soundings be started considerably earlier to allow the balloon to reach its top height (20 kilometers) or the airplane to reach its ceiling height (6 kilometers) and to bring back its record. In the event that the start of a sounding is delayed for any reason, such as unfavorable flying conditions in the case of the airplane soundings, the data are not available in time for use in forecasting. Such loss of data is particularly serious in the case of outpost stations, such as Sault Ste. Marie, Michigan. These are key stations in the Weather Bureau network.

The Sault Ste. Marie station stands as an outpost sentinal to give information concerning the development of cold waves which affect the thickly-settled and highly-industrialized eastern part of the United States. Regularity of data from Sault Ste. Marie is of particular importance because cold waves are usually associated with fast action in the atmospheric flow and can occur with great and sometimes unexpected suddenness. It is precisely at such a station that adverse flying weather occurs with considerable frequency and serves to postpone or cancel airplane soundings. The use of radio sondes at this station has practically eliminated interruptions in the regularity of observations. Even at a station such as Oakland, where weather conditions are usually favor-

able to flying, the regularity of observations with the radio sonde surpasses that for airplane soundings for corresponding seasonal periods.

The increased range of observations possible with the radio sonde is apparent from the ceiling height of the balloon, 20 kilometers, as compared with the ceiling height of the airplane, 6 kilometers. There is some loss (in the radio sonde method) of observations such as may be made by the pilot in an airplane sounding; for example, the heights of cloud bases and tops, haze layers, and levels where ice formation takes place; however, such information may be derived indirectly from the radio sonde data. The greater height attained and the increased regularity of observation far outweigh this disadvantage.

An added advantage for the radio sonde method is pointed out on the basis of the reduction in total time required for a sounding. At the present time, a balloon rate of ascent of 200 meters per minute is employed so that 100 minutes is required to obtain information to 20 kilometers. A new humidity device with a faster response is now under development at the Bureau, and when it is adopted the rate of balloon ascent may be doubled, reducing the time for a complete sounding to 50 minutes. The average airplane sounding to a height of only 6 kilometers requires about 90 minutes.

STRATOSPHERE ULTRAVIOLET METER.

A new radio stratosphere ultraviolet meter which operates with a precision comparable with that of laboratory instruments is described in RP1181 by R. Stair, in the March *Journal of Research*. The improved instrument consists of a photoelectric cell, a balanced amplifier, a relaxation oscillator, and a transmitter. The calibration of the instrument is automatically determined and broadcast to the receiving station each time an altitude signal is given. The instrument, with accessories, including sufficient batteries to operate for about four hours, weighs about five pounds. It operates on a frequency of approximately 50 megacycles and has sufficient power to insure good reception for distances from the receiving station up to 100 miles.

PREPARATION OF OXYGEN OF HIGH PURITY.

The Bureau coöperates with other national physical laboratories in establishing the International Temperature Scale. This Scale is the reference standard against which all measurements of temperature are ultimately compared. One of the defined points of this scale is the normal boiling point of pure oxygen, −182.97° C. RP1182, by Martin Shepherd, E. R. Weaver, and S. F. Pickering, which will be published in the *Journal of Research* for March, describes the preparation of oxygen of high purity which was used as one of the reference samples in the determination of the oxygen point of this temperature scale.

ESTABLISHMENT OF A LOW-TEMPERATURE SCALE FOR CALIBRATING THERMOMETERS.

Above −190° C. thermometers are calibrated at the Bureau on the basis of the International Temperature Scale—the scale used by most of the standardizing laboratories of the world. This scale does not extend below −190° C. and, heretofore, the Bureau has not calibrated thermometers for the public below this temperature. It has been necessary for laboratories desiring to make precise temperature measurements between −252° and −190° C. to calibrate their thermometers by comparison with a gas thermometer. Not only is this time consuming, but it is not satisfactory because the precision of gas thermometry is not high, and different laboratories have established different secondary scales of temperature based upon their calibrations of their resistance thermometers or thermocouples.

The use that is being made of temperatures below −190° C. for both scientific and industrial purposes is steadily increasing and as a consequence the need for the calibration of thermometers for this low range is growing. In order to promote scientific research and the industrial application of temperatures below −190° C., a temperature scale extending from −260° to −190° C. has recently been established at the Bureau for the calibration of thermometers.

In setting up this scale, Harold J. Hoge and F. G. Brickwedde compared seven carefully annealed resistance thermometers (six platinum and one platinum −10 per cent.

rhodium) with a gas thermometer. In the future, thermometers submitted for calibration will be compared with these calibrated resistance thermometers. At present the newly established scale is maintained by the seven resistance thermometers. However, it is intended that the scale shall be based upon fixed points, that is, boiling, triple, and transition points of pure substances, and determinations are now being made of the temperatures of fixed points on the newly established scale. The temperatures found for the boiling and triple points of normal hydrogen on this scale are 13.96° and 20.39° K. (−259.20° and −252.77° C.) respectively.

EFFECT OF THE SOLUBILITY OF GLASS ON THE BEHAVIOR OF THE GLASS ELECTRODE.

The reported voltage departures of the glass electrode in (1) the majority of alkaline solutions above pH 8.5, (2) high concentrations of acid solutions, (3) concentrated salt solutions, (4) ethanol solutions and (5) for elevated temperatures have been studied by Donald Hubbard, Edgar H. Hamilton, and Alfred N. Finn in connection with the solubility of the glass customarily used for making glass electrodes. As reported in RP1187 (*Journal of Research* for March), for every case investigated voltage departures of the glass electrode have been found to be accompanied by changes in the solubility of the glass.

COMPARISON OF 2, 6-DIMETHYLHEPTANE WITH ISONONANE.

In the separation of the constituents of petroleum which is being carried on at the Bureau by the American Petroleum Institute Research Project 6, there was isolated several years ago, from a midcontinent petroleum, an isononane normally boiling at 135.2° C. On the basis of the meager data then available, the isononane was tentatively identified as 2, 6-dimethylheptane. Now Joseph D. White, F. W. Rose, Jr., and G. Calingaert have synthesized 2, 6-dimethylheptane by means of the Grignard reaction, and have compared the properties of a purified fraction with those of the isononane from petroleum. The properties of the isononane are in good accord with those of 2, 6-dimethylheptane. The properties

of the latter (extrapolated to a purity of 100.0 per cent. from measurements actually made on material of purity 99.6 mole per cent.) are as follows: Boiling point at 760 mm. Hg 135.21 ± 0.02° C.; freezing point in air, −102.95 ± 0.10° C.; density at 20° C., 0.70891 ± 0.00003 g./ml.; refractive index, n_D^{20}, 1.40073 ± 0.00005; critical solution temperature in aniline, 80.0 ± 0.3° C.

RP1184 in the March *Journal of Research* should be consulted for a complete account of this work.

FLUIDITY TEST FOR QUALITY OF CELLULOSE FIBERS.

One of the most sensitive indications of the quality of cotton, rayon, and other cellulose fibers is the apparent fluidity of a cuprammonium solution in which a definite amount of the fiber in question is dispersed. The fluidity test has been studied recently by R. T. Mease in the Bureau's Textile Section who gives detailed directions for making it in the *Journal of Research* for March (RP1179).

The test is particularly applicable for the measurement of small changes in cellulose resulting from the action of light, laundering, mildew, bacteria, acids, bleaching agents and the like. It should, therefore, be of general interest.

In making the test, the weighed sample of cellulose is dissolved in a cuprammonium solution carefully standardized to contain 240 ± 5 g. of ammonia per liter, 15 ± 0.1 g. of copper, and less than 0.5 g. of nitrites and nitrates calculated as nitrous acid. Dispersion of the cellulose is effected in a closed tube to prevent loss of ammonia. The rate at which the solution flows from the orifice of the tube is observed. The apparent fluidity of the liquid is calculated from the calibration of the tube.

The paper describes the equipment that is used at the Bureau in making the test. Although it has been specially designed for the purpose it can be readily constructed in most laboratories. The precautions to be taken in the preparation and storage of the cuprammonium solution are discussed. The construction and calibration of suitable viscometers are described and data are given showing the duplicability of results with different viscometers and with the same viscometer.

DOPED FABRICS FOR AIRCRAFT.

Despite the extensive use of metal construction in the fabrication of modern commercial and military aircraft, doped fabric continues to meet a need for a light and relatively inexpensive covering for low-stressed members and for the wings and fuselages of small airplanes. The "dope," which is applied to the fabric to make it impermeable to air and to weatherproof it, consists of a film-forming material dissolved in organic solvents. Cellulose nitrate is at present commonly used as the film-forming base, but it is easily ignited and the rate of burning is very rapid. The Bureau of Aeronautics of the Navy Department, therefore, requested the Bureau to develop a nonflammable dope which would compare favorably with or surpass cellulose nitrate dope with respect to the effects of normal aging and high relative humidity on the tautness of the doped fabric.

In a previous report (*J. Research N.B.S.*, **20**, 651; May, 1938 (RP1098)) it was shown that the solvents and diluents in a dope govern to a large extent its tautening properties and the durability of the film deposited on the fabric. The four variables involved in the formulation of airplane dopes are the type of plastic, plasticizer, solvent, and diluent. A second report by F. W. Reinhart and G. M. Kline of the Organic Plastics Section, which will be published in Industrial and Engineering Chemistry, presents the results of an investigation of the effect of these variables upon the properties of films formed from various plastics. The plastics examined were cellulose acetate, cellulose triacetate, hydroxypropylcellulose triacetate, cellulose acetopropionate, cellulose acetobutyrate, cellulose nitrate, ethylcellulose, benzylcellulose, chlorinated rubber, methyl methacrylate resin, and isobutyl methacrylate resin. Data are included on the solubility of these plastics in common organic solvents and mixtures of solvents and diluents, their compatibility with various plasticizers, and the degree of flexibility, clarity, and shrinkage of the films prepared from the plasticized and nonplasticized compositions.

On the basis of the information obtained with the single solvents and solvent-diluent combinations, various mixtures were formulated in an endeavor to obtain flexible, clear films

characterized by marked shrinkage, qualities which are of prime importance in a good airplane dope. This process of selective formulation and testing was continued for each material until no further improvement in the desired film properties was noted. The solutions chosen in this manner, with the addition of selected plasticizers, have been applied to fabric-covered test panels to determine their tautening properties and the weathering characteristics of the deposited film. Exposure and flammability tests on these experimental panels are now under way.

PRINTING TESTS OF EXPERIMENTAL BOOK PAPERS.

During the past four years the extent to which composition and manufacturing processes affect the stability of paper has been investigated at the Bureau. The influence of filling and sizing materials on book paper was recently reported. Tests to evaluate the printing quality of the experimental papers manufactured in that investigation have since been made. The additional data reported by Merle B. Shaw and Robert H. Simmons in the *Journal of Research* for March (RP1180), consist of laboratory measurements of comparative smoothness, oil penetrability, and air permeability at the Bureau, and of actual printing trials at the Government Printing Office.

As a group, the precipitated calcium carbonate-filled papers had the best printing quality. Little difference attributable to fiber was shown in the printing quality of the papers made from old rags, sulfite-soda, or purified-wood book pulps, all being satisfactory. The test results apply only to the materials used, which were representative at the time the work was done. The relationship may be changed with further improvement in quality of the materials as a result of advances in manufacturing technic.

Opacity and smoothness are known to be related to printing quality. Less is known of the relation of oil penetration and air permeability, but it is believed that they also are indicative of the behavior of paper in actual printing. As data on the tests are accumulated and correlated with quality of printing the tests should be of value in paper specifications.

THE FRANKLIN INSTITUTE

STATED MONTHLY MEETING, MARCH 15, 1939.

The regular monthly meeting of The Franklin Institute was called to order at 8:30 o'clock, by Mr. W. Chattin Wetherill, Vice-President.

The Chairman called upon the Secretary for his monthly statement.

He reported the additions to membership since the last report as follows:

Active	11
Associate	9
Student	60
Total	80

He also announced that the April demonstration in the Planetarium would be: "A Trip to the Moon," and that Television would be shown soon in the Museum.

The Chairman then recognized Dr. Thomas D. Cope, Chairman of the Boyden Premium Committee, who gave a brief account of the founding of this premium, in 1859, which was intended as an award to any resident of North America who shall determine by experiment whether all rays of light, and other physical rays, are or are not transmitted with the same velocity. He referred to the work of Dr. Paul R. Heyl in 1905 and 1906 in Central High School in Philadelphia, who was recognized in 1907 by the award of a portion of this fund for the solution of the problem so far as the transmission of the visible ultra-violet rays is concerned. Dr. Cope then outlined the work of Dr. John S. Hall of Amherst College Observatory, whose recent Memoir was recommended for the award of this premium. Dr. Hall has shown that visible light of wave-length 5500 Angstroms and infra-red light of wave-length 8600 Angstroms travel with velocities that are identical. Dr. Hall was then presented to the Chairman who delivered to him a copy of the report of the Committee with the award.

Dr. Hall on receiving the documents expressed his appreciation for the honor conferred on him.

The Chairman announced that Dr. Paul R. Heyl of Washington, the recipient of the first award of this Foundation, was present at the meeting and requested him to rise.

Dr. Dugald C. Jackson, Professor Emeritus of Electrical Engineering, Massachusetts Institute of Technology, was then introduced. He presented a paper entitled: "Man in an Engineering World." The speaker discussed the changes in man's life arising from scientific discoveries and engineering inventions; these were taken up and examined over the expanse of time within the historical period. Everyone is affected by the application to life of scientific discoveries through inventions, and the lecturer pointed out some of the changes produced, viewing them in a spiritual rather than in the usual material way.

At the close of the paper, the Chairman conveyed the thanks of the meeting to Dr. Jackson for his interesting discussion.

Adjourned.

HENRY BUTLER ALLEN,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(Abstract of Proceedings of Stated Meeting held Wednesday, March 8, 1939.)

HALL OF THE COMMITTEE,
PHILADELPHIA, MARCH 8, 1939.

MR. WILLIAM G. ELLIS *in the Chair.*

The following reports were presented for final action:

No. 3034: Work of George A. Campbell.

This report recommended the award of an Elliott Cresson Medal to George Ashley Campbell, of Upper Montclair, New Jersey, "In consideration of his life-long study of the theory of electric circuits, resulting in notable contributions to the science that underlies telephony and to inventions of fundamental importance in the art."

No. 3058: Work of John R. Carson.

This report recommended the award of an Elliott Cresson Medal to John R. Carson, of New York City, New York, "In consideration of outstanding contributions to the art of electrical communication."

No. 3067: Levy Medal.

This report recommended the award of the Louis E. Levy Medal to Kalman J. DeJuhasz, of State College, Pennsylvania, for his article entitled "Graphical Analysis of Surges in Mechanical Springs," which appeared in the October and November, 1938, issues of the JOURNAL of the Institute.

JOHN FRAZER,
Secretary to the Committee.

LIBRARY NOTES.

The Committee on Library desires to add to the collections of the Institute any technical writings of members who have had occasion to publish such material.

Literary contributions from author-members will be gratefully acknowledged, properly inscribed and noted in the Journal of the Institute.

Photostat Service. Photostat prints of any material in the collections can be supplied on request. Orders received in the morning are filled the same day. The average cost for a print $8\frac{1}{2} \times 11$ inches is thirty-five cents.

The library and reading room are open on Mondays, Tuesdays, Fridays and Saturdays from nine o'clock A.M. until five o'clock P.M., Wednesdays and Thursdays two until ten o'clock P.M. 801 readers made use of the facilities during the twenty-four days of February.

RECENT ADDITIONS.

ASTRONOMY.

ST. JOHN, CHARLES E., AND OTHERS. Revision of Rowland's Preliminary Table of Solar Spectrum Wave-Lengths. 1938.

WOOLLEY, R. VAN DER RIET. A Key to the Stars. 1935.

BIBLIOGRAPHY.

BUROS, OSCAR KRISEN, Editor. Research and Statistical Methodology. Books and Reviews, 1933–1938. 1938.

BIOGRAPHY.

FAŸ, BERNARD. The Two Franklins: Fathers of American Democracy. 1938.

KERMACK, W. O., AND P. EGGLETON. The Stuff We're Made Of. 1938.

LEONARDO DA VINCI. The Notebooks of Leonardo da Vinci, Arranged, Rendered into English and Introduced by Edward MacCurdy. Two volumes. No date.

MCKIE, DOUGLAS. Antoine Lavoisier, the Father of Modern Chemistry. No date.

POGGENDORFF, J. C. Biographisch-literarisches Handwörterbuch. Bd. vi, 3 Teil. 1938.

BIOLOGY AND BIOCHEMISTRY.

MELLANBY, E., AND L. RUZICKA, Editors. Ergebnisse der Vitamin-und Hormonforschung. Band 2. 1939.

CHEMISTRY AND CHEMICAL TECHNOLOGY.

BENNETT, H., Editor. The Chemical Formulary. Volume 4, 1939.

BÖMER, A., A. JUCKENACK, AND J. TILLMANS, Editors. Handbuch der Lebensmittelchemie. Achter Band, Erster Teil. 1939.

DROSSBACH, PAUL. Elektrochemie geschmolzener Salze. 1938.

FINDLAY, ALEXANDER. The Phase Rule and Its Applications. Eighth Edition. 1938.

Food Industries Manual. 1938.

FRIEND, J. NEWTON, Editor. A Text-book of Inorganic Chemistry. Volume 6, part 4. 1938.

HAUSER, ERNST A. Colloidal Phenomena. 1939.

LIPPY, JOHN D., JR. Chemical Magic. No date.

RICHTER, VICTOR VON. The Chemistry of the Carbon Compounds. Volume 2. 1939.

SWIFT, ERNEST H. A System of Chemical Analysis. 1939.

TWYMAN, F. Spectrochemical Abstracts, 1933–1937. 1938.

TWYMAN, F. Spectrochemical Analysis in 1938. 1938.

WILSON, CECIL L. An Introduction to Microchemical Methods. 1938.

DICTIONARIES.

Cassell's New German-English Dictionary. 1936.

EDUCATION.

RAMSEY, GRACE FISHER. Educational Work in Museums of the United States. 1938.

ELECTRICITY AND ELECTRICAL ENGINEERING.

BROWN, O. F., AND E. L. GARDINER. The Elements of Radio-Communication. Second Edition. 1939.

HAGUE, B. Alternating Current Bridge Methods. Fourth Edition. 1938.

MOULLIN, E. B. Spontaneous Fluctuations of Voltage. 1938.

VEINOTT, CYRIL G. Fractional Horsepower Electric Motors. First Edition. 1939.

ENGINEERING.

American Society for Testing Materials. Proceedings of the 41st Annual Meeting, 1938. Volume 38, parts 1–2. 1939.

HUDSON, RALPH G. The Engineers' Manual. Second Edition. 1939.

International Commission on Large Dams of the World Power Conference. Second Congress on Large Dams. Washington, 1936. Transactions. Five volumes. 1938.

GEOLOGY.

EMMONS, WILLIAM H., AND OTHERS. Geology: Principles and Processes. Second Edition. 1939.

PORTER, HOLLIS P. Petroleum Dictionary. Second Edition. 1939.

MANUFACTURES.

CURTIS, HARRY P. The Testing of Yarns and Fabrics. Third Edition. 1938.

KRAUSE, HUGO. Metal Coloring and Finishing. Translation from the Second Revised Edition. 1938.

WAKELING, ARTHUR, Editor. Things to Make in your Home Workshop. 1939.

MATHEMATICS.

FRAZER, R. A., W. J. DUNCAN, AND A. R. COLLAR. Elementary Matrices. 1938.

MECHANICAL ENGINEERING.

BEVAN, THOMAS. The Theory of Machines. 1939.

METALLURGY.

CARPENTER, SIR HAROLD, AND J. M. ROBERTSON. Metals. Volumes 1–2. 1939.

NAVAL ARCHITECTURE.

HORST, CLAUDE WILLIAM. Model Sail and Power Boats. Revised and Enlarged. 1939.

RAILROADS AND TRANSPORTATION.

RECK, FRANKLIN M. The Romance of American Transportation. 1938.

PHOTOGRAPHY.

American Photographic Publishing Company. The Modern Encyclopedia of Photography. Two Volumes. No date.

CRABTREE, J. I., AND G. E. MATTHEWS. Photographic Chemicals and Solutions. 1939.

STADE, GERHARD, AND HERBERT STAUDE. Mikrophotographie. 1939.

PHYSICS.

HEIL, HERMAN G., AND WILLARD H. BENNETT. Fundamental Principles of Physics. 1938.

SCIENTIFIC ESSAYS.

BELL, ERIC T. Man and His Lifebelts. 1938.

GEORGE, WILLIAM HERBERT. The Scientist in Action. 1938.

WOLF, A. A History of Science, Technology, and Philosophy in the Eighteenth Century. 1939.

NOTES FROM THE BARTOL RESEARCH FOUNDATION.

GEIGER-MÜLLER COUNTER MEASUREMENTS OF COSMIC-RAY INTENSITIES IN THE STRATOSPHERE.*

BY

W. F. G. SWANN, G. L. LOCHER and W. E. DANFORTH.

We are here reporting data concerning cosmic-ray intensities in the stratosphere, as measured during the three following manned balloon flights: (1) that of Professor and Mrs. Jean Piccard from Detroit, September, 1934, (2) that sponsored by the National Geographic Society and the U. S. Army Air Corps (Explorer I) from Rapid City, South Dakota, in July, 1934, and (3) the Second National Geographic flight (Explorer II) in November, 1935. These results were reported at the symposium held in connection with the April, 1936, meeting of the American Physical Society in Washington; and a complete description of the apparatus and experiments will be found elsewhere.[1]

Relative intensities as a function of distance below the top of the homogeneous water equivalent atmosphere are given, for several zenith angles, in Fig. 1. These were obtained in Explorer II by a triple coincidence counter telescope system provided with a suitably designed wall of shielding counters to eliminate spurious coincidences resulting from showers.

The intensity maximum, occurring at about 82 cm of water for the vertical component, is probably the most important feature of these data.[2] The initial rise of the curve

* Reprinted from the *Physical Review*, 51, 389 (1937).

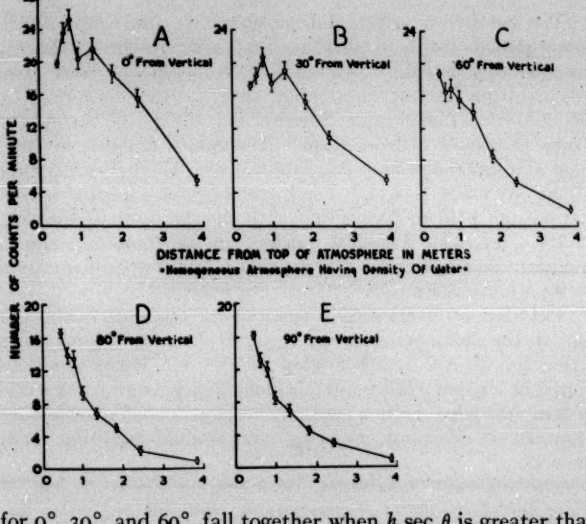
[1] W. F. G. Swann and G. L. Locher, J. FRANK. INST., 221, 275 (1936); W. F. G. Swann, J. FRANK. INST., 222, 23–58 (1936); W. F. G. Swann, J. FRANK. INST., 222, 669–680 (1936); also W. F. G. Swann, G. L. Locher and W. E. Danforth, *Nat. Geog. Soc. Contributed Tech. Papers*, Stratosphere Series, No. 2 (1936).

[2] Our apparatus was installed and tested ready for flight, and the inflation was proceeding on July 11, 1935, when the top of the balloon blew off. The successful flight with the same apparatus was made on Nov. 11, 1935. In the interval, the intensity maximum was also discovered and reported by Regener (*Nature*, 136, 718 (1935)).

with increase of distance below the top of the atmosphere can, we believe, be ascribed to the production of secondaries whose number increases with thickness of atmosphere available to produce them. A suggestion of a secondary hump is indicated between 100 and 200 cm depth.

If one plots the data for different zenith angles (θ) as a function of h sec θ, where h is the vertical depth, the curves

FIG. 1.

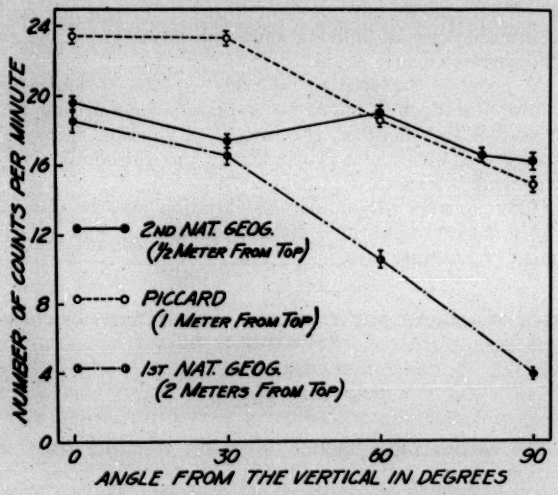


for 0°, 30°, and 60°, fall together when h sec θ is greater than 200 cm, a result to be expected if the intensity depends only upon distance traveled in a straight line through the atmosphere. However, the intensities found at 80° and 90° are much greater than the values necessary to harmonize with the above laws. It is believed that such failure results from the influence of the earth's magnetic field in relation to the paths of the secondaries.

Curves showing the angular distribution at different altitudes are given in Fig. 2. Noteworthy here is a progressive

flattening of the distribution as the altitude is increased, and a remarkably high horizontal intensity at the highest altitude. Care must be exercised in the interpretation of these curves, however, for, at high altitudes (particularly for large zenith angles) a countertelescope receives rays from both ends under conditions such as those prevailing, conditions in which the path lengths of the secondaries traveling in the rarified atmosphere can suffer appreciable bending by the earth's magnetic field.

FIG. 2.



Data from the different flights as to the relative increase of the radiation with altitude (referred to sea level) are as follows. At 40,000 ft. the Explorer I gave us a factor of 38; the Explorer II, 36. At 53,000 ft. our equipment in Dr. Piccard's gondola showed a factor of 44, while that in the Explorer II gave 42. At 57,000 ft, the Explorer II gave 51 (the maximum), and decreased from that point on to a ratio of 41 at 72,400 ft.

To compare these data with those obtained with ionization

chambers we integrated the intensity, graphically, over all angles concerned. The large relative increase in the oblique (high-zenith angle) radiation serves to make the integrated intensity increase more rapidly with altitude than does the vertical component.[3] But still the counter detected increase with altitude falls short of that found with the ionization chamber. Whereas we find a 100-fold increase in the integrated counter radiation at 57,000 ft., the ionization measurements of I. S. Bowen, R. A. Millikan and H. V. Neher [4] in the Settle-Fordney stratosphere flight give a factor of about 200. That this should represent a real difference in cosmic-ray intensity seems unlikely since the latitudes of the two flights were so nearly equal.

A possible explanation of this counter-*vs.*-ionization-chamber discrepancy might be secondary rays arising from the wall of the chamber, the rate of production thereof increasing with altitude in proportion to the soft component of the general radiation.

Other matters whose altitude variation may be involved in this disagreement are: specific ionization, and relative number of multiple rays.

EFFECT OF GALACTIC ROTATION UPON THE INTENSITY OF COSMIC RADIATION.*

BY

W. F. G. SWANN.

The earth's rapid motion in space, resulting from the rotation of our galaxy, results in variations in cosmic-ray intensity fore and aft of the earth's motion. This effect has been discussed, and calculations of its magnitude have been made by A. H. Compton and I. A. Getting [1] and by A. H. Compton.[2]

[3] The uncertainty in the number of counter impulses received from the two ends of the counter telescope becomes eliminated by the integration in its effect upon the calculated ionization.

[4] I. S. Bowen, R. A. Millikan and H. V. Neher, *Phys. Rev.*, **46**, 641 (1934).

* Reprinted from the *Physical Review*, **51**, 718 (1937).

[1] A. H. Compton and I. A. Getting, *Phys. Rev.*, **47**, 817 (1935).

[2] A. H. Compton, *Phys. Rev.*, **50**, 1119 (1936).

The theory of the matter assumes a different form for neutral particles than for charged particles; and the calculation in the latter case is recognized as complicated and is not given explicitly. The purpose of the present paper is to formulate the solution for charged particles in a manner which is precise and general, although it is true that the actual calculation of the effect involves knowledge of the orbits concerned. A secondary purpose of the paper is to call attention to certain features which may have a simplifying effect upon the picture of the whole problem. We shall not concern ourselves here with the effect of the atmosphere, which effect has been discussed in detail by A. H. Compton. We shall confine ourselves to the intensity of the primary particles entering the atmosphere.

Our problem is the determination at a point O on the surface of the earth, of the cosmic-ray intensity in a direction towards O, parallel to a radius vector inclined at an angle θ to the velocity v of the point O, which velocity v shall be taken in the positive direction of the axis of x. The intensity may, of course, also be a function of the azimuth φ, and, in the direction concerned is defined as I, where

$$dN = I \sin \theta d\theta d\varphi ds, \qquad (1)$$

where dN is the number of particles falling per second within the limits of the solid angle $\sin \theta d\theta d\varphi$ upon the element of area ds drawn perpendicular to the radius vector. We shall use dashed letters to denote measurements made in the system moving with velocity v; and, it will, therefore, be in terms of dashed letters that the quantity we desire will in general be expressed.

Now, in the case of a charged particle moving in a pure magnetic field (i.e., without electric field) it follows from a theorem analogous to Liouville's dynamical theorem, that the intensity in the above-defined sense is constant along a dynamical path when measured, of course, in the direction of that path, or rather in the opposite direction.[3] This result is

[3] This result was first stated by G. Lemaitre and M. S. Vallarta, *Phys. Rev.*, **43**, 87 (1933). Their proof suffered from certain logical imperfections which really restricted it to a case where the magnetic field was absent. The proof developed subsequently by the present writer (W. F. G. Swann, *Phys. Rev.*, **44**, 224 (1933)) removed these defects, and established, in place of Liouville's theorem,

true in a system of axes fixed to the moving earth on account of the invariance of the Hamiltonian equations which control the dynamics of the problem.[4] Thus, in seeking the intensity in a given direction at a point on the earth's surface, we must trace the orbit back to infinity. The intensity we desire will be the intensity along this orbit at infinity estimated in the moving system; but we must obtain it through the relativity transformation as applied to a condition in which, in the fixed set of axes, the intensity is constant for all positions and directions at infinity. The conclusion reached is true for charged or uncharged particles, the only difference being that, for uncharged particles the direction of the orbit at the point O is the same as at infinity, so that the intensity at O is determined by the direction at O, while for charged particles it is determined by the different direction which the orbit assumes at infinity.

Let us then discuss the intensity in the direction defined by θ', φ', at infinity in the moving system S'.

The number of particles dN, in addition to being given by (1) is also given by

$$dN = u\delta\rho ds, \tag{2}$$

where u is the velocity in S of the particles which are moving parallel to the direction defined by θ, φ, and $\delta\rho$ is the contribution to the density in S by those particles which travel within the limits $d\theta d\varphi$. Thus, from (1) and (2)

$$I = u\delta\rho/\sin\theta d\theta d\varphi; \text{ and, of course,}$$
$$I' = u'\delta\rho'/\sin\theta' d\theta' d\varphi'. \tag{3}$$

Now if $\epsilon \equiv (1 - v^2/c^2)^{-1/2}$; $k \equiv (1 - u^2/c^2)^{-1/2}$; $\beta = v/c$ and if $E(= m_0 k)$ refers to the total energy of the particle, we have, as 4 − vectors, (ku_x, ku_y, ku_z, kic) and $(u_x\delta\rho, u_y\delta\rho, u_z\delta\rho, ic\delta\rho)$.

From the above relations, combined with the fact that

a sort of corollary of the general form of that theorem, but with the noncanonical variables required by the cosmic-ray problem.

[4] As a matter of fact, the result would not be true for charged particles in a stationary set of axes for the case of a moving earth, since, in that set of axes, the moving magnetized earth would produce an electric field which was the equivalent of an electric doublet. This electric field would result in changes of energy along the dynamical path. In fact, while the energy is constant along a dynamical path when measured in one set of axes, it is not constant along that path when measured in another relatively moving set of axes.

$\sin^2\theta = (u_y^2 + u_z^2)/(u_x^2 + u_y^2 + u_z^2)$, and with the assumption that $|u|$ is nearly equal to c, the velocity of light, and that quantities involving β^2 and higher powers are negligible, it readily follows that

$$\sin\theta' d\theta' = (1 - \beta\cos\theta')^2 \sin\theta d\theta; \qquad d\varphi' = d\varphi;$$
$$u'\delta\rho' = u\delta\rho/(1 - \beta\cos\theta').$$

Hence, as given by Compton and Getting,

$$I' = I/(1 - \beta\cos\theta')^3. \tag{4}$$

In accordance with the above remarks, therefore, if we desire the intensity in a certain direction at the point O, just outside the atmosphere, we must trace the trajectory which ends in that direction back to infinity, and there find the angle θ', which it makes with the direction of the earth's motion. With this value of θ', the intensity I' is given by (4) in terms of the standard uniform intensity I applicable for all directions at infinity in a stationary system. This statement is true for uncharged particles as well as for charged particles. For the former, however, the trajectories are straight and θ' is the same for the trajectory at infinity as it is at the earth's surface.

A further matter operative in the case of charged particles concerns the alteration of the numbers of particles which can reach the earth in opposition to the earth's magnetic influence. From the relativistic transformation for the energy we have $E' = \epsilon E(1 - vu_x/c)$, or, neglecting v^2/c^2, and recognizing that $|u|$ and θ are very nearly equal to c, and θ' respectively, we have

$$E' = E(1 + \beta\cos\theta'). \tag{5}$$

Now if $F(E)dE$ is the fraction of the rays which have energies between E and $E + dE$ at infinity in the resting system, the entering intensity involves an integral of $F(E)dE$ to infinity from a lower limit E_θ, characteristic of the magnetic latitude. In the moving system, the integral will be from $E' = E_\theta$ to infinity, and therefore, from $E_\theta/(1 + \beta\cos\theta')$ to infinity. Thus, the range of integration is increased by $\beta E_\theta \cos\theta'$ (or, of course, reduced if $\cos\theta'$ is negative), and the intensity is altered by the fraction $\beta E_\theta F(E_\theta)\cos\theta'$ of its value.

Here E is characterized by the angle between v and the reversed path of the trajectory at O, while θ' is characterized by the angle between v and the reversed path of the same trajectory at infinity. In the case where $F(E)$ is of the form A/E^n, the above fraction becomes $(n-1)\beta\cos\theta'$, and for n of the order 2 or 3,[5] becomes comparable with the effects of the other kinds already discussed.

Reverting to the more obvious of the foregoing effects, that concerned with the impact of noncharged particles upon a moving earth, it is perhaps worthwhile to clarify one point which may cause difficulty if not appreciated, although it is, of course, properly taken care of in the foregoing discussion, and applies to charged particles as well. Suppose we confine our attention to the simple case where a plane perpendicular to the direction of the axis of x is moving along the positive direction of that axis with velocity v, and where parallel rays of velocity u are falling vertically upon it. The most naïve view of the phenomenon represents a picture in which the number of rays received per second per square centimeter of the plane is $\rho(u-v)$, when the plane is moving with velocity v, and ρu when it is stationary, so that the ratio is $(u-v)/u$. In this naïve picture, the relative velocity is altered by the motion from u to $u-v$, and ρ is unaltered. However, when u is nearly equal to c, the velocity of light, it follows from the theory of relativity that the relative velocity is not appreciably altered by the translatory velocity v of the system. In fact, observers in all systems specified by constant velocities v measure the same value for the velocity of a particle when in any one of them that velocity is equal to c. This is true whether the particle is an ordinary particle or a light particle. Our naïve expression $\rho(u-v)$ is now replaced by $\rho' u$; but, ρ' is no longer equal to ρ, but to $\epsilon\rho(1-uv/c^2)$ which, for u nearly equal to c is $\rho(1-v/c)=\rho(1-v/u)$. Hence $\rho' u=\rho(u-v)$ as for the naïve case, and the ratio of this quantity to that for a stationary plane is, as before, $(u-v)/u$. Now, however, the alteration is entirely due to a change in the apparent density resulting from the motion, whereas in the naïve picture it was caused by a change in relative velocity without change of density.

[5] See W. F. G. Swann, *Phys. Rev.*, **50**, 1103 (1936).

NOTES FROM THE BIOCHEMICAL RESEARCH FOUNDATION.

The Director's 1938 progress report outlined in general terms the work that is being done in this laboratory along lines of ultracentrifuge development. It was explained that some of the most promising applications of the air-driven type of ultracentrifuge lie in biological fields; here it has been found to be extremely useful in obtaining proteins of high molecular weight in a purified state from body fluids. A notable example is found in sedimentation by means of the ultracentrifuge of the protective antibody from antipneumococcus serum obtained from immunized horses. This antibody has the property of associating from a molecular weight of 156,000 in undiluted serum to 950,000 in serum diluted three to four times with physiological saline. The heaviest of the inert protein components in the diluted serum however has a molecular weight of 156,000. This differential in molecular weights of active antibody and inert protein permits considerable concentration and purification of the antibody through sedimentation in the ultracentrifuge. The adaptation of this type of apparatus to practical fractionation requires that the fluid containing the component to be isolated be passed continuously through the ultracentrifuge.

Investigations on the separation of protein components from blood serum have suggested that the continuous separation type of ultracentrifuge can be used as a fractionating tool. It was evident that any useful application leading to a routine procedure would be seriously retarded unless it was paralleled by ultracentrifuge development. For the continuous separation type of ultracentrifuge to be operated efficiently in a routine it is necessary that the system of controls be adjusted to reduce the personal element in the operation of the ultracentrifuge to a minimum. Even more important is the necessity for adequate knowledge of the strength of materials of the key parts of the apparatus. For unless this is appreciated, one must operate the ultracentri-

fuge well below its ultimate efficiency or risk failure through faulty knowledge of the upper speed limit of the rotor.

In the case of a tubular steel rotor of given dimensions it is usually possible to calculate the theoretical speed at which the rotor will burst. Unfortunately, however, steels of very high tensile strength have little elasticity. Consequently a rotor made from one of these steels is apt to become fatigued after several runs at high speed. In the fatigued condition its strength is unknown and it is liable to burst at speeds much lower than those previously withstood. Considerable experimentation is being carried on to determine the best possible ultracentrifuge rotor for continuous separation in biological work. Choice of steel alloy for rotor material is to be governed by the property of fatigue resistance.

Ultracentrifuge Development.—THOMAS J. DIETZ. (Physico-Chemical Department of the Biochemical Research Foundation of the Franklin Institute, Philadelphia, Pa.) The development of the continuous separation air-driven ultracentrifuge by Beams has contributed materially to the potentialities of ultracentrifugation as a fractionation procedure, and serves to carry commercial centrifuge methods into the macromolecular range. The continuous separation type of ultracentrifuge is being applied in this department to the fractionation of fluids of biological origin. Throughout the work emphasis is being placed on simplicity and efficiency of fractionation, since the adaptation of ultracentrifuge methods to commercial demands is dependent upon economic feasibility. Consequently the work has naturally expanded into two distinct phases: (1) that of devising methods of preparing and handling biological fluids to obtain desirable components in a purified state; and (2) that of developing the continuous separation ultracentrifuge to a degree where its operation is compatible in efficiency with the economic factor involved.

Investigations in the former of the two phases mentioned above are built upon the following premise. Utilizing intense centrifugal force it is possible to concentrate and purify a disperse component from a fluid medium by means of a specific or semi-specific aggregating agent when the molecular weight of the unassociated disperse component is too small

to make differential sedimentation profitable. While it is contended that the premise can be adjusted to wide applicability, it has been necessary in this work to confine activities to proteins, an important constituent of vital systems. Experiments designed to explore the possible applications of the above mentioned premise to the fractionation of protein constituents indicate that certain proteins can be aggregated so as to permit substantially specific sedimentation in the ultracentrifuge. The present incomplete condition of the work permits description only in qualified terms. However, from our work to date, it is believed that for certain proteins one can choose an aggregating agent in the form of another protein, to be added in such concentration as to bring about the formation of primary aggregates whose chemical activity, insofar as desired prosthetic groups are concerned, has not been excessively diminished. This manipulation presupposes a multivalency of both the protein to be aggregated and the aggregating agent. It also demands that, if the desired aggregated condition of the protein is merely a transient state in an equilibrium reaction, the state be prolonged sufficiently to permit the fractionation.

Preliminary experiments suggest that it will also be possible, at least under specialized conditions, to bring about controlled aggregation by means of some of the methods ordinarily used for precipitating proteins. Although formaldehyde is not an ordinary protein precipitant the following example serves to indicate the relationship. Electrophoretic mobility measurements made on centrifuged and control mixtures of Type I antipneumococcus serum (horse) and formaldehyde (dilute) seem to support the idea that a primary effect of the formaldehyde on the serum is to cause a preferential aggregation of the immune component of the serum globulin, while slightly higher concentrations of formaldehyde bring about indiscriminate aggregation of all of the globulin components. This too is mentioned only in general terms, for while a specific aggregation may occur in principle, to apply it as an economically feasible manipulation depends upon the conditions specified for the system.

Certain unpredictable inconsistencies in results were experienced when reacting components were obtained or pre-

pared in a variable manner. For example, it was possible under certain specified conditions to bring about an aggregation of the immune globulin component in Type I antipneumococcus serum (horse) utilizing the type-specific bacterial polysaccharide as an aggregating agent. When a second polysaccharide, identical insofar as type-specificity was concerned but prepared under different conditions, was employed the results were negative. The second preparation of polysaccharide aggregated a small fraction of the antibody to form a visible precipitate leaving the excess apparently unaltered in the supernatant liquid. Since controlled aggregation depends predominantly upon physico-chemical characteristics of the reacting components, it was decided that sedimentation and electrophoresis data would be used wherever possible to standardize the aggregating components.

Early experiments showed that the development of the continuous separation ultracentrifuge must progress with the development of fractionation methods utilizing the ultracentrifuge. As this project was aimed at extending the field of usefulness and increasing the efficiency of this type of ultracentrifuge it was obvious that the upper limit of efficiency was dependent upon the design of the rotor and the strength of materials from which the rotor was made. It works out that steel is the most satisfactory rotor material and it was decided that the important question to settle in development is the choice of the most suitable steel alloy. A second question which necessarily affects the first is that of the fatigue characteristics of the alloy used. Since the steel will doubtless become fatigued under the repeated centrifugal stress, it is necessary to be able to predict the life of a rotor in continuous use. Otherwise there is no knowledge of the fatigue of the rotor and consequently a hazard is introduced. Svedberg mentions his choice of steel for routine use for analytical ultracentrifuge rotors as a ductile chromium-nickel steel alloy, saying that while the harder steels could withstand higher rotational speeds they were not dependable. This lack of dependability is probably the result of fatigue and can possibly be controlled if the fatigue condition of the steel can be defined properly both before and after each run.

In the continuous separation ultracentrifuge, the rotor is

tubular (the axis of rotation being the axis of the tube) and, because of its shape, is particularly adaptable to strength of material studies. Test rotors in this shape can be machined without difficulty and are comparatively inexpensive. Consequently, as mentioned in the second paragraph of this note, an investigation, designed to determine the most satisfactory characteristics for rotor steel with regard to ultimate tensile strength, ductility, and fatigue characteristics, constitutes the second phase of this work. Experiments under way at present include bursting tests on nine different types of steel alloys. The nine are divided into three groups of three steels each: group I, tensile strength—280,000 lbs. per sq. in. or greater, reduction of area—30 per cent. or less, and Brinell hardness—500 or greater; group II, tensile strength—220,000 to 280,000 lbs. per sq. in., reduction of area—30 per cent. to 50 per cent., and Brinell hardness—400 to 500; group III, tensile strength—220,000 lbs. per sq. in. or less, reduction of area—50 per cent. or greater, and Brinell hardness—400 or less. Steels in group I are chosen for high tensile strength even at the expense of ductility, in group II the highest possible combination of tensile strength and ductility is chosen, and in group III high ductility is obtained at some sacrifice of tensile strength. One type of steel in each group is classed as corrosion-resisting. The corrosion-resisting steels are included until considerably more experience has been gained with the various steels. Corrosion resistance is judged to be an important factor in fatigue strength of the steel, and the question as to whether it is necessary to use corrosion-resisting steel or merely to protect the surface in contact with the fluid by chromium or gold plating is yet to be answered. It is known that, in systems in which a thick walled tube is subject to internal pressure, failure of the tube begins at the inner surface of the wall, so particular care is being taken in machining to a mirror finish the inner surface of the experimental rotor tubes.

The importance assumed by fatigue characteristics of rotor steels in routine operation of a continuous separation ultracentrifuge demands that an accurate check be kept on the fatigue condition of the steel whenever the rotor is spun up beyond a safe limit. Therefore experiments are being in-

cluded in this study of rotor steels for the purpose of developing a suitable method for measuring some characteristics of the rotor from which the condition of fatigue can be inferred. These experiments are designed to measure variation in magnetic permeability of the experimental rotors while they are under stress.

In view of the various points cited in this note with regard to development in two phases, it seems logical to expect that any fractionation method utilizing ultracentrifugation must depend for its practical application upon the safe and inexpensive operation of the ultracentrifuge as a routine procedure.

BOOK REVIEWS.

THE SCIENTIST IN ACTION, by William H. George. 354 pages, illustrations, 14 × 23 cms. New York, Emerson Books, Inc. Price $3.00.

In the usual course of things, science is an existing subject in itself. Any individual who follows this subject as a collector of truths, or partial truths about this subject, is a scientist, and the results of the work of scientists are being applied in practical affairs to the extent of affecting human happiness and the altering of civilization. Yet, few of the users of these results, or even scientists themselves, have ever considered the scientist as a part of the subject of science, to the point of placing the subject secondary to the man. In this book science is taken as the activities of a scientist, and the form of human action rather than the way of thinking about things is scientific research. Scientific research is not in itself a science; it is an art or craft.

To carry out the program of the author, the four qualities of scientific research first selected for general discussion are (1) action which gives two typical products, (2) the first of these being facts, (3) the second being such arrangements or patterns of fact as scientific classifications, laws, and theories, and (4) an essential quality of newness which often does not fit into a current theory of pattern of facts.

Continuing along these lines, difference is shown with the standard treatment of common action in everyday life as due to the adoption of a fundamentally different attitude, and not to differences in detail or application of an attitude shared in common. This is reflected in the so-called Attack-Escape principle, that on most occasions when most adults are first conscious of something new they either attack or try to escape from it. Again, the research worker makes no absolute statements about how things should or should not appear to behave. It is barriers like these, that seem to act against the extensive use of research technique.

A division is made into two parts of all elements of consciousness, one concerning sense data got by judgments of coincidence (coincidence observations), and the other containing all that is not observable by this judgment of coincidence, such as thinking, believing, etc., and consciousness of emotions. This division is regarded as separate but parallel throughout the book. Research work is a double process of using sense organs to observe and of using imagination and pure reason to build up ideas or mental concepts into which the observations can be fitted. The statement that "Science is based upon measurement" is rejected in favor of the form "Science is based upon fact" (or human judgment of coincidence).

Considerable discussion is devoted to observation and the resulting selection of facts which precedes the analysis of a wide range of human actions in terms of the conception that one of the characteristics of normal human actions is that they fit into or make a complete pattern. Problems of theorizing, the tech-

nique of experiment, and the personal bias are among other pertinent topics treated.

The book is an analysis of the research worker in the light of a biological machine. The author suggests that, since the traditional methods of dealing with those very important social problems created by the application of scientific research are failing, a study of scientific action as here presented may be a method of solution. It is the only untried method at present known.

R. H. OPPERMANN.

RESEARCH AND STATISTICAL METHODOLOGY, Books and Reviews of 1933–1938, edited by Oscar Krisen Buros. 100 pages, 16 × 24 cms. New Brunswick, Rutgers University Press, 1938. Price $1.25.

In recent years new methods have been discovered for accomplishing what here-to-fore was done in a comparatively crude manner in the realm of statistical analysis. It is therefore most important, that those working in this field keep abreast of the times. To do this is not an easy thing, for reviews of books which may evaluate these methods are scattered among numerous journals in different fields. The author of this book attempts to provide assistance by giving here excerpts from book reviews of books on research and statistical methodology. It is patterned after a Rutgers University Bulletin—Vol. 14, No. 2a; Studies in Education, No. 11—a section of which is devoted to a book review digest. This, however, is primarily devoted to books for students of education and psychology, and covers the period 1933–36. The present book has been expanded to include books and monographs published between Jan. 1, 1933 to Nov. 15, 1938. Excerpts are not repeated in this volume, but excerpts of the additional books for the entire period are. The reviews noted are chosen from a rather broad field of periodicals which publish reviews made by persons familiar with the subject. The excerpts are carefully separated. At the beginning, there is a classified index to the reviews which gives a first division of reviews on research methodology and then statistical methods. Under the former, such subdivisions as business and economics, engineering, scientific method, and social sciences may be found, and under the latter graphic calculations, medical and vital statistics, and tables.

Anyone in search of the latest in the fields covered by this book would do well to give it a try.

R. H. OPPERMANN.

A SYSTEM OF CHEMICAL ANALYSIS FOR THE COMMON ELEMENTS, by Ernest H. Swift. 589 pages, illustrations, tables, 16 × 23 cms. New York, Prentice-Hall, Inc., 1939. Price $4.25.

The analytical laboratory is constantly called upon for the solution of problems of wider and wider range, consequently, as it rises to meet these demands the more of industrial importance it becomes. Of late, there has been a large variety of materials of all kinds, as well as a multitude of instruments and equipment, all new, presented to facilitate and improve laboratory technique. Truly, the chemist must need be alive to these opportunities. But not only have these decended upon the laboratory worker; the very foundation of methods and

practices are questioned and revised in the interest of time saving and greater applicability to pressing problems. The book at hand goes into this phase. It is an exposition of a system of analysis that, it is stated, is the result of an experimental investigation by the author and numerous associates covering a period of over ten years. The object is to provide reliable and sensible qualitative information in regard to the constituents present in the material being analyzed and sufficient quantitative information to eliminate in many cases the need for further quantitative determinations.

The system is one in which volumetric methods are used extensively. Differentiating this from gravimetric methods, the final weighing of a compound of definite percentage composition is replaced by the measurement of volume of a standard solution which reacts directly or indirectly with the substance being estimated. The book accordingly, starts with a treatment on this subject and proceeds through precipitation, oxidation and reduction, permanganate, and iodometric methods of volumetric analysis. The preparation and standardization of solutions are clearly described, and reference is made to the applications of the methods. A similar arrangement is laid out for the neutralization and displacement methods which are often designated as acidimetry and alkalimetry, and which can be used for the estimation of the acids and bases, the estimation of salts and weak acids or of weak bases, and the indirect determination of other substances which can be converted into or stoichiometrically precipitated by, compounds of this type. While the gravimetric methods are put aside in favor of the volumetric, the author recognizes certain advantages of the former and uses it on occasions by reference. To round out completeness of the work there is a discussion of these principles and a description of the technique. This completes the first part of the book.

The second part of the book treats on the system of analysis for the basic constituents. Here the preparation of the sample, the separation of the basic constituents into groups, the precipitation separation and analysis of; first the hydrogen sulfide, copper and tin groups; and second the ammonium sulfide, aluminum and zinc groups, and the alkaline earth group, are described. In this section there is sought the detection of certain elements without regard to their state of oxidation or combination in the original material. In part three, in addition to the detection of certain elements, the analysis of the acidic constituents has for its aim definite information as to these states. The problem is accordingly made quite complicated. The treatment is an attempt to provide a scheme for acidic analysis approaching the systematic nature of that for the basic elements. The treatment is not complete in this respect because of the complex nature of the problem brought on by the possibility that one or more of the constituents may exist in the same solution under certain conditions. However, the present state of the subject is well covered and the shortcomings are surmounted by providing supplementary tests for certain individual constituents.

The book has been used for several years in mimeographed form as the text for the course in analytical chemistry in the sophomore year at the California Institute of Technology. The treatment is clear and practical, and is supplemented with useful data for teacher and student in appendixes. In the back there is a list of equipment and a comprehensive subject index.

R. H. OPPERMANN.

FUNDAMENTAL PRINCIPLES OF PHYSICS, by Herman G. Heil and Willard H. Bennett. 631 pages, illustrations, tables, 16 × 24 cms. New York, Prentice-Hall, Inc., 1938. Price $3.75.

There are many reasons why there are available a number of texts on this subject. Modern presentations with new methods of approach; the preparation for the study of, as well as the exposition of new thoughts and ideas; the arrangement of the subject matter to conform with changes in manner of teaching the subject; all of these justify a new text to fit the particular case or cases. The book at hand, while being up-to-date in many respects, has for its main purpose a construction that lends itself to the simultaneous teaching of the calculus. As the student progresses from the beginning, gradual use of the calculus is made until such time arrives when a comprehensive vision of it has been obtained. The advantages of such a method are obvious, namely the accomplishing of two ends, the allowance of time for the study of the calculus and the use of it in the solution of practical problems in physics.

Aside from this, the book generally is an up-to-date physics text. There are some 562 pages covering the topics of mechanics and heat; wave motion, sound and light; electricity and magnetism; and atomic physics; in this order. It is illustrated with drawings usually found in such works, as well as curves. There are a number of tables some of which are ingeniously conceived to make simpler the point or property under discussion. Exercises and problems appear throughout and many of these are worked out to show the procedure. A helpful hint is often given with problems for exercise where it is deemed necessary.

The book presents the subject in a clear, concise, and complete manner. It may be used to advantage by those taking formal courses, those desiring to obtain a knowledge of the subject via home study, and those who wish a reference work within the limits of the title.

R. H. OPPERMANN.

ELEMENTARY MATRICES AND SOME APPLICATIONS TO DYNAMICS AND DIFFERENTIAL EQUATIONS, by R. A. Frazer, W. J. Duncan and A. R. Collar. 416 pages, illustrations, tables, 16 × 25 cms. Cambridge, The University Press, 1938. Price $8.50.

To appreciate any subject to the fullest extent requires its study from a carefully prepared course. Everything possible must be considered and done to make definite and lasting impressions, not only of as many of the important points as can be made, but of the dynamics of each—their evaluation and applicability. Nowhere does this apply more than in the subject of mathematics and nowhere do fundamentals play a more important role. The topic of matrices is one which, if mastered, has valuable application in calculation. This book develops the topic from its fundamentals, and therefore is subject to the double spotlight of examination.

Written for students of applied mathematics who have no previous knowledge of matrices, it begins with an explanation of what matrices are, how they may be used in simple arrangements, some of the properties of matrices which are expressible as functions of a given matrix or which have elements functionally dependent on real or complex parameters, development of ideas of differentiation

and integration of matrices and a treatment of certain types of infinite series of matrices which define the exponential function. Then there is given an account of some of the properties of matrices which have for their elements polynomials of a parameter which is followed by an explanation of the important conception of the equivalence of matrices, and a brief review of some of the canonical forms to which matrices can be reduced. The computation of determinants, reciprocal matrices, and adjoint matrices, and with the numerical solution of systems of linear algebraic equations; the limiting forms of high powers of matrices; the approximate calculation of latent roots; the approximate of algebraic equations of general degree and the computation of Sturm's functions and test functions for stability; are all considered in this part of the book devoted to properties which are later required for applications to dynamics and differential equations.

The remainder deals with applications. This starts with the solution of systems of linear ordinary differential equations and proceeds through an indication of application in the approximate solution of differential equations, kinematics and dynamics of systems, motions governed by linear ordinary differential equations with constant coefficients which is followed by a discussion and exemplification of approximate numerical methods of solution, dynamical systems with solid friction, applications of friction theory to flutter problems, and finally pitching oscillations of a frictionally constrained aerofoil. Some of this matter, not yet completely understood, consists of a brief description of some experimental tests.

The exposition, plan, and approach used in the book are good. The method of giving worked numerical examples is to be commended. The book would fit the practical mathematician with special regard to the applications to differential equations and classical mechanics.

R. H. OPPERMANN.

MAN AND HIS LIFEBELTS, by Eric T. Bell. 340 pages, 15 × 22 cms. Baltimore, The Williams & Wilkins Company, 1938. Price $3.00.

During a period of quiet thoughtfulness, have you ever pondered over the question of what there is for us, as individuals and the human race generally, to look forward to in the way of saving us from the inevitable end of extinction? It only takes a little reflection to see clearly that the things we have which are the result of the workings of the human mind, both tangible and intangible, certainly have contributed a little to the continuation of human life beyond a measurable length of time. These things, Mr. Bell calls lifebelts—something we can cling to for our preservation and salvation. His book, as the title implies, is a discussion and review of them, as well as a presentation of conclusions which he has reached with regard to what is being done and what should be done about them.

Naturally very prominent among these lifebelts is science, which from its very beginnings has produced wonders for the betterment of life, increasing the span of life, and probing further and further into the mysteries of the unknown. Yet, as the author points out, engines of destruction have been and will be ingeniously made that threaten oblivion. Invention itself, instead of civilizing our race, precipitated the Industrial Revolution. Then there is the lifebelt of religion, which according to Mr. Bell, has let its adherents down in this life, as

well as in ages past, although with regard to its effect on the behavior of the masses, the present do not believe as their forefathers believed. Another great lifebelt is that of government, and here the present condition of the world in this respect is laid out to speak for itself. Education and the science of economics are likewise treated.

And so the story proceeds. On those subjects with which the reader is not intimately familiar, the book adds to what depressed condition he may already be in. And on many with which he is familiar, anger may be aroused because of reference to subjects and conditions where the reader may disagree. Generally the tone is one of pulling down rather than of building up, in an attempt to reveal facts in their true light. In other words the reader should be prepared to experience a series of emotions, at least a different one on every page. The method of writing includes language of the highest as well as going down to almost slang. There seems to be no limitation in this and it extends into catch phrases and even long sentences quite beside the point so that in a great many places the reader must re-read paragraphs in order not to loose the thread of progression. Readers of the class that could follow Jeans' Mysterious Universe would have little trouble here, however.

But there is one thing that the book has, beyond any doubt. No matter what your last impression has been on pausing, you are drawn back to reading more.

R. H. OPPERMANN.

PHOTOGRAPHIC CHEMICALS AND SOLUTIONS, by J. I. Crabtree and G. E. Matthews. 360 pages, illustrations, 17 × 25 cms. Boston, American Photographic Publishing Company, 1939. Price $4.00.

Photography, whether practiced by amateurs or professionals may generally be considered more of an art than a science. Yet, the underlying principles and even much of the technique are entirely scientific. In the use of chemicals and the fixing of solutions, very good results can be obtained by following printed directions, but the careful worker certainly realizes as essential the value of a knowledge of the properties of the chemicals used and chemical reactions involved. The book at hand has just this for its purpose—to give an understanding of the fundamental principles of solution preparation and use.

It is written in a practical way that any worker can follow and covers the subject quite completely. At the outset, terminology and arrangement of photographic formulas is discussed with photographic arithmetic and a set of explained tables of conversion formulas. Then comes apparatus and methods of using them. This includes various types of scales and supplementary equipment, mixing tanks, stirrers, agitators, filters, drying cabinets, etc. A fund of time saving information is contained in the description of materials for construction of photographic processing apparatus and temperature measurement and control. There is given considerable data on what extent impurities in water may be harmful to the various operations and how these impurities may be removed, the technique of mixing and using photographic solutions, and handling solutions at high temperatures. The photographer who has been in the situation when he has run out of stock of a certain chemical and desired to conveniently substitute another, should be interested in what is said here on the limits of such

substitution. Stains on negatives and prints, their classification and removal; the removal of stains from hands and clothing; general suggestions and precautions on handling photographic chemicals; are other useful topics covered. A formulary for each branch of the photographic industry in the United States and other very useful information completes the book.

Photographers of all grades are sure to find valuable information here. The book is a well written presentation, easily read by any worker. It is a manual of practice and a good, quick reference work.

R. H. OPPERMANN.

THE ENGINEERS' MANUAL, by Ralph G. Hudson. Second Edition, 340 pages, illustrations, tables, 13 × 20 cms. New York, John Wiley & Sons, Inc., 1939.

Have you ever searched through numerous reference works for a particular formula which you have used in the past? If you have, then you have experienced the trouble and loss of time in locating it, if you have found it at all. Many engineers retain their personal college days note book just for this purpose, and of course with the passage of time much of this material becomes obsolete or the book becomes worn with use. The author of "The Engineers' Manual" has endeavored here to provide the answer in a convenient pocket size book. Any engineer on paging through it will recognize much of value in an easy-to-get-at form, and even recall certain useful formulas which have been forgotten. As a matter of fact, in many cases, this book should relieve the student of a great deal of note making.

The book is in its second edition, the entire chapter on Heat and a large part of the chapter on Electricity having been rewritten and brought up to date. The subjects covered by this edition include mathematics from algebra through trigonometry, analytic geometry, the calculus, vectors, and hyperbolic functions; mechanics including kinematics, relations of mass and space, kinetics, statics, beams, steel bars, and shafts; hydraulics with formulas on hydrostatics and dynamics, and flow; heat, its transmission, flow of gases and steam calorimeters; electricity covering direct and alternating currents, with sub-classes under these general headings. The back of the book contains tables of data on these subjects, some of them quite unusual in scope.

Engineers who come in contact with any of this subject matter, would do well to brouse through this book and then test its value by using the index.

R. H. OPPERMANN.

THE MOBILITY OF POSITIVE IONS IN GASES, by A. M. Tyndall. 93 pages, illustrations, tables, 14 × 22 cms. New York, The Macmillan Company, 1938. Price $1.75.

This little book is one of the Cambridge Physical Tracts previously mentioned in these columns as being composed of authoritative accounts of subjects of topical interest. The author in this case has been directly associated with the subject and has obtained results which have lead to definite conclusions. The work outlines definitely from early investigations to date the position of the subject and some aspects of it. Beginning with the results obtained at atmospheric and reduced pressures, it proceeds through measurement, glow discharge and hot surface methods of ion production for the study of mobility, the assump-

tions of Langevin and Hasse' with regard to the force of interaction between the ion and gas molecule, and experimental data obtained in the Bristol Laboratory with which the author is connected. The latter is especially interesting because the work here performed reveals satisfactory agreement between the experimental and theoretical values for the heavier gases, but in the case of a light gas like helium the agreement is not so good. Of further interest is the information on the relation between mobility, field and pressure and the effect of polar impurities. The work ends with a discussion of that important problem in atmospheric electricity and meteorology, the large ion.

The account recorded is a distinct contribution to knowledge of the subject.

R. H. OPPERMANN.

STEEL AND ITS HEAT TREATMENT, by D. K. Bullens. Volume II—Engineering and Special-Purpose Steels. Fourth Edition, rewritten and reset. 491 pages, illustrations, tables, 16 × 24 cms. New York, John Wiley & Sons, Inc., 1939. Price $5.00.

Volume I of this set was recently mentioned in these columns. This covered the principles, processes and fundamentals of control of procedures vital to heat treatment. Volume II, the book at hand, is devoted to a discussion on the outstanding properties of the most-used steels with some attention to other steels that might be of value but are not yet standard products. The picture presented is a unified one rather than a detailed one.

There are three sections to the book. The first is on simple engineering steels covering carbon, phosphorous, sulphur, lead, manganese, silicon, copper, nickel and chromium steels. Of each of these a description is given of composition, treatment and general properties and special stress is laid on the alloying element being bolstered up by small amounts of other elements in order to impart to the steel some desirable attribute not sufficiently provided by the single element. This leads to a discussion on what is called assistant elements or those which exert strong effects but which are normally utilized only in small amounts. These are aluminum, titanium, zirconium, vanadium, etc. and it is shown how they may be utilized because of their effect upon grain size, their influence on depth hardening, or their ability to enter into complex carbides, to harden these carbides and to stabilize them for high temperature use. Then plain molybdenum and tungsten steels, and the comparison of alloying effects—S.A.E. steels and treatments are given attention.

Engineering steels of more complex composition is the heading of the second section of the book. Here high yield strength, low alloy steels are discussed which is followed by complex alloy steels based on ferrite formers and those based on carbide formers, the former including the nickel-chromium and similar families and the latter, chromium and similar families. Special mention to alloy steels for carburizing, their selection, the core properties and distortion in carburizing is given and to alloy steels for nitriding. A very interesting presentation is that on spring steel properties. This section is concluded with a treatment on steel castings and a brief consideration of the problem of a choice among the constructional alloy steels.

Part three is devoted to special purpose steels which begins with steels for high temperature service and proceeds through the Austenitic Steels, carbon

tool steels, low and intermediate alloy tool and die steels, high speed tool steels, and permanent magnet steels and their heat treatment.

The material of which the book is composed is obtained from references given at the end of each chapter. The reference material is very well analyzed, classified and edited so that the book is a smooth running account. The amount of work involved must have been tremendous, and there is ample evidence that it was expertly performed.

There is an extraordinary amount of data in the form of tables and curves. The subject index covers Vol. I and Vol. II.

R. H. OPPERMANN.

RADIO TROUBLE-SHOOTER'S HANDBOOK, by Alfred A. Ghirardi. First Edition, 518 pages, illustrations, 23 × 29 cms. New York, Radio & Technical Publishing Co., 1939. Price $3.00.

The layman who tries to determine the cause of trouble in his radio receiver has no difficulty in coming to the early conclusion that he has an exceedingly difficult job on his hands. He can readily appreciate the radio serviceman's work particularly so when considering that there are thousands of receiver models and makes. To do this work quickly and effectively requires even more than wide experience, it requires a mass of reference data that is up-to-date and in a readily usable form.

The author of this book is well known to the serviceman for his text on radio servicing and he offers here a supplement to that 1300 page volume. It gives that information which is a constant and important necessity in service work. There are 52 sections to the book, the largest of which is the first, containing case histories on over 3300 models. It gives in a condensed, tabulated form the accumulated information from some thousands of hours of experience in service work. This includes how to recognize troubles by their symptoms and how to correct them. Then there is an intermediate peak frequency listing, that has been factory checked to insure accuracy. All sorts of other useful information is given. Some examples are how to eliminate persistent auto-radio ignition interference, how to use exponents in radio calculations, operating characteristic data and socket connection charts for tubes which may be used as replacements for obsolete and non-standard type tubes, directory of receiver manufacturers, etc.

The arrangement of the material in this book certainly is such that enables quick use. This together with the wealth of data contained in it, should make it valuable to everyone in the business.

R. H. OPPERMANN.

PHYSICS, A TEXTBOOK FOR COLLEGES, by Oscar M. Stewart. Third Edition, 750 pages, plates, illustrations, 16 × 24 cms. Boston, Ginn and Company, 1938. Price $4.00.

The many changes in our mode of living in the past one hundred years have been the result of a more widespread use of scientific methods of thinking. These methods have become so important and so useful that every one should know more about them. In these days, one who does not understand them cannot be said to have a good education. This is particularly true in the case of the scientific man—so true that he cannot hope for any measurable degree of success without

their practice. Further, the development of a basic subject like physics is fast reaching a point where it is becoming necessary to understand more of the nature and philosophy of the methods which are being used. It should, therefore, be a continuous underlying purpose in the mind of every author of a physics text, to so guide the reader as to impress him.

This book is in its third edition, the first two having been used widely. It is an elementary treatment of college grade and reflects experience in observing response in the use of the earlier editions with special reference to the scientific methods. The coverage includes mechanics and properties of matter, heat, wave motion and sound, magnetism and electricity, and light. The presentation is done in an exceptionally clear style and it is uniform throughout. The author takes great pains in maintaining interest, especially in the introduction which appears at the beginning of each chapter. Innumerable devices are used here—history, every-day observations of physical phenomena, present activities in research and their remarkable results, etc. At the end of many chapters a summary is given which is of the type ordinarily appearing in the form of notes taken during lecture periods. The chapters are short, giving a carefully measured amount of material which is supplemented by a long list of problems. The answers to the problems are given in the back of the book. Worked out problems may be found in the text where it was deemed necessary to assure understanding.

The book is up-to-date in subject matter and may be classed with the best examples of modern texts. A comprehensive subject index makes it useful as a reference, as well as a text to gain a knowledge of the fundamentals of the subject.

R. H. OPPERMANN.

NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS.

Report No. 640, The Aerodynamic Characteristics of Full-Scale Propellers Having 2, 3, and 4 Blades of Clark Y and R. A. F. 6 Airfoil Sections, by Edwin P. Hartman and David Biermann. 23 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1938. Price 10 cents.

Aerodynamic tests were made of seven full-scale 10-foot-diameter propellers of recent design comprising three groups. The first group was composed of three propellers having Clark Y airfoil sections and the second group was composed of three propellers having R. A. F. 6 airfoil sections, the propellers of each group having 2, 3, and 4 blades. The third group was composed of two propellers, the 2-blade propeller taken from the second group and another propeller having the same airfoil section and number of blades but with the width and thickness 50 per cent greater. The tests of these propellers reveal the effect of changes in solidity resulting either from increasing the number of blades or from increasing the blade width.

It was found that (1) increasing the solidity by adding blades had a lesser adverse effect than increasing it by increasing the blade width; (2) the loss in efficiency commonly conceived to be the result of increasing the number of blades was not fully realized, only about 2 per cent difference in peak efficiency between a 2-blade and a 4-blade propeller being measured; and (3) an increase in solidity tended to delay the stall and to increase the efficiency in the take-off range.

Propeller design charts and methods of computing propeller thrust are included.

Report No. 648, Design Charts for Predicting Downwash Angles and Wake Characteristics Behind Plain and Flapped Wings, by Abe Silverstein and S. Katzoff. 25 pages, illustrations, 23 × 29 cms. Washington, Government Printing Office, 1939. Price 15 cents.

Equations and design charts are given for predicting the downwash angles and the wake characteristics for power-off conditions behind plain and flapped wings of the types used in modern design practice. The downwash charts cover the cases of elliptical wings and wings of taper ratios 1, 2, 3, and 5, with aspect ratios of 6, 9, and 12, having flaps covering 0, 40, 70, and 100 per cent of the span. Curves of the span load distributions for all these cases are included. Data on the lift and the drag of flapped airfoil sections and curves for finding the contribution of the flap to the total wing lift for different types of flap and for the entire range of flap spans are also included. The wake width and the distribution of dynamic pressure across the wake are given in terms of the profile-drag coefficient and the distance behind the wing. A method of estimating the wake position is also given.

The equations and the charts are based on theory that has been shown in a previous report to be in agreement with experiment.

SOME EPISODES IN THE MANCHESTER ASSOCIATION OF ENGINEERS, compiled by A. C. Dean. 139 pages, 14 × 22 cms. Manchester, Association, 1938.

The name of the Manchester Association of Engineers indicates a purely local body with local interests. It is well recognized however that such a body will in time exert a wide influence and become known at considerable distances through the work of the members connected with it as well as the work of the association itself from which the members benefit. Still another, but no less important reason is the environment of the association. In the case of Manchester, all of these have so played a part, that, the association is not by any means unknown the world over. This little book will certainly do its share in this respect. It is composed of extracts from the early minute books which give an interesting picture of the manner in which the affairs were conducted and the interest and pride that was taken in the engineering profession. A special feature is the recordings relative to the Manchester Ship Canal and reports of discussion meetings, the latter covering a great variety of subjects.

R. H. OPPERMANN.

TENSOR ANALYSIS OF NETWORKS, by Gabriel Kron. 635 pages, illustrations, 16 × 24 cms. New York, John Wiley & Sons, Inc., 1939. Price $7.50.

Almost every practicing electrical engineer is familiar with the mental and physical drudgery that accompanies the solution of many types of problems. Along with this comes the ever present possibility of errors in calculations and manipulations of equations. It is therefore quite natural for those encountering such problems to be watchful for a tool that will facilitate the work. The author here suggests the use of tensors which have been known also as hypercomplex numbers, polyadics, matrices, etc. The solution of problems with the aid of these

concepts has been accomplished to some extent for some years by engineers in the profession, but their theory and use today is by no means widespread, so they still may justly be considered new.

One of the reasons why tensors have not been generally accepted is because being a mathematical method, they have been presented in the literature from the point of view of the professional mathematician rather than the engineer. In recognition of this, the author of this book very definitely states that the book is for engineers who are interested in learning an organized method of attack to analyze and synthetize electrical networks. The presentation in this light consists of a development of the theory of tensors in-so-far as is necessary, the investigation of the setting up of the equations of voltage of a mesh network, the showing of various examples where transformation of currents involves changes of various types either in the physical set up of the system or in the selection of the variables encountered, the explanation of how reasonings and equations established for mesh networks can be repeated in dual form for junction networks, the organization of a conglomeration of tensors having different valencies into entities with more complex structure called multiple tensors, and various types of network problems. There is also explained how some of the physical concepts and analytical processes introduced can be illustrated in a geometrical language and how geometric objects may be subdivided into several parts. There is developed reduction formulas to speed up the labor of reducing the number of linear equations or of solving them and to facilitate the physical analysis accompanying them. With regard to the synthesis of networks there is considerable information. These and many other aspects of the subject are given careful exposition.

The greater part of this treatment may be absorbed by anyone who has a knowledge of elementary mathematics and some acquaintance with Ohm's and Kirchhoff's laws. The construction of the book is flexible with regard to the type of reader. Its application to readers of different types is quite well summarized in the introduction where it tells readers of various interests what chapters to follow which will provide ready and quick information. The book will undoubtedly do much toward the wider use of tensors in engineering.

R. H. OPPERMANN.

THE PRINCIPLES OF ELECTROCHEMISTRY, by Duncan A. MacInnes. 478 pages, illustrations, 16 × 24 cms. New York, Reinhold Publishing Corporation, 1939. $6.00.

Electrochemistry is a subject which, as the compound word indicates, lies between the specific confines of two definite fields. They are chemistry proper where chemical changes occur, and electricity, the fundamental basis of which is physics. Some may prefer to go back to the old assertion that physics is the basis of all, but this would lead to differences of opinion that always arise when such a stand is taken. For the purposes of this book the author defines electrochemistry to include the chemical changes which occur in systems when electrical energy either appears in or disappears from their surroundings. Further, the greatest emphasis is laid on the electrochemical phenomena taking place at the electrical conducting boundary areas between metal-electrolyte, and electrolyte-electrolyte. With this in mind the presentation is an account of modern theoretical electrochemistry, in a logical connected whole.

The introduction contains a brief history from early knowledge to the present. The treatment begins with a statement of Faraday's Law and Coulometers, where the passage of electricity in electrolytic conductors is illustrated in the way of particles. This is followed by the study of the total effect of the presence of the ions on the conductance, and the proportions of the total current which are carried by the different varieties of ions. Thermodynamics is then introduced, and those portions of it that are of interest in electrochemistry are presented, such as that connected with galvanic cells and the theory of solutions, also in the exchanges of chemical substances and in energy between a system and its surroundings. The Debye-Hückel method for the theoretical calculation of activity coefficients is explained. The predictions are afterwards used in comparison with those activity coefficients determined with the aid of various types of concentration cells including cells without transference containing amalgam electrodes, cells with transference, and cells without transference containing mixtures of electrolytes.

At this point cases are demonstrated showing the effects of gravity and centrifugal force on the electromotive force of galvanic cells. Subsequently the subjects of standard potentials, thermodynamic studies of non-aqueous solutions, determination and meaning of "pH" values, potentiometric titrations, the effect of structure and substitution on the ionization constants of organic acids and bases, electrokinetic phenomena, and irreversible phenomena are discussed. An appendix gives the derivation of Plank's equation for liquid junction potentials. There is an index of authors as well as a subject index.

The author has chosen and arranged his material wisely so that it is a smooth flowing account. Illustrative of the limitations of the diversified topics which are contained in the general subject, is that of thermodynamics which is carefully molded and fitted well into its place. Little space in the book is wasted on anything but what is necessary to accomplish the purpose. The book is a presentation of the present aspects of the subject in a manner that should make it valuable to all those interested in this field.

R. H. OPPERMANN.

THE STRUCTURE AND COMPOSITION OF FOODS, by Andrew L. Winton and Kate Barber Winton. Volume 4, Sugar, Sirup, Honey, Tea, Coffee, Cocoa, Spices, Extracts, Yeast, Baking Powder. 580 pages, illustrations, 16 × 24 cms. New York, John Wiley & Sons, Inc., 1939. Price $9.00.

This is a subject that holds the interest of workers in a number of fields. To name a few, perhaps biological chemistry is of prime importance and this is followed by agriculture, manufacturing, trade, and even economics. It is not difficult for the layman to understand the broad significance of the subject. Those directly interested will at once recognize the potentialities of it and how the position of present knowledge will be enhanced with an authoritative work containing in one place the accumulated results of research from a broad front.

The book is Volume IV of a series. It covers the subjects of sugar, sirup, honey, tea, coffee, cocoa, spices, extracts, yeast, and baking powder. It is so constructed as to be encyclopedic in nature. For instance, under the general heading of sugar cane products there is first given a description of sugar cane

(Saccharum officinarum L.), where it is cultivated, its appearance, crossing and selection of the plant, and the percentage of sucrose. Then the manufacture of raw sugar is described followed by sugar refining, chemical composition, molasses, and constituents. The other subjects are likewise treated. Every effort is made to give as much data as possible which is recorded in numerous tables as well as interesting detailed illustrations.

There are some 530 pages comprising the subject matter. It represents a tremendous amount of work in compiling data from the literature of the world, foreign and domestic, and so arranging it as to present it in a logical sequence in each of the subjects covered. The work gives ample evidence of an expert knowledge coupled with a sense of orderliness. The comprehensive index in the back adds greatly to the value of this book as a reference tool.

R. H. OPPERMANN.

GRIMSEHLS LEHRBUCH DER PHYSIK, neubearbeitet von Prof. Dr. R. Tomaschek. Erster Band, 681 pages, illustrations, 15 × 23 cms. Leipzig, B. G. Teubner, 1938. Price 19.80 R.M.

Other volumes of this set of German textbooks have been reviewed in these columns before. The January number of the JOURNAL mentioned one devoted to the electromagnetic field and optics, which was the eighth edition. The book at hand is the tenth edition of Volume I of the set. As progress is made in the various topics covered, a new edition of any one of the books comprising the set is published. This accounts for the different edition numbers in the same series and also insures that the set containing the highest edition numbers has the most up-to-date material. On what has already been said of these various books, it is unnecessary to enlarge, with respect to their being an authoritative standard in the German language.

This book covers the general subjects of mechanics, heat, acoustics and ballistics in an elementary manner. It contains some 680 pages of text with a great many illustrations. The order of progression is headed with a treatment on various types of physical measuring apparatus and from this goes into kinematics, power, gravitation, and an investigation of the fine structure of matter especially in-so-far as it becomes plainly evident in elementary physics. This is followed by elasticity, friction, an extraordinarily plain exposition with regard to liquids and gases and their flow. At this point is a good illustration of the manner of treatment throughout the book. The physics of the operation of modern aircraft is admirably explained for both heavier and lighter than air machines. The subjects of oscillations and waves are likewise given attention. Heat, acoustics and ballistics complete the book.

Some of the improvements made in the book include greater emphasis on molecular relationship in surface tension, suction pumps, heat equivalents, and an accurate treatment on spin momentum. The method of presentation is one which gives lasting impressions and the usual thoroughness is much in evidence. A name and subject index is located in the back.

This book can be recommended for reference to those who can read German and who desire information on fundamentals of the subjects covered.

R. H. OPPERMANN.

PUBLICATIONS RECEIVED.

Physical Meteorology, by John G. Albright. 392 pages, plates, illustrations, 16 × 24 cms. New York, Prentice-Hall, Inc., 1939. Price $5.35.

Direct-Current Machinery, by Hempstead S. Bull. 318 pages, illustrations, 15 × 24 cms. New York, John Wiley & Sons, Inc., 1939. Price $3.00.

A Short History of the Steam Engine, by H. W. Dickinson. 255 pages, plates, illustrations, 16 × 25 cms. New York, The Macmillan Company. Price $3.50.

Telegraphie et Telephonie sans Fil, by G. Gutton. 9e Edition entierement refondue. 178 pages, illustrations, 11 × 17 cms. Paris, Armand Colin, 1939. Price 15 fr.

The Application of Tensors to the Analysis of Rotating Electrical Machinery, by Gabriel Kron. Parts I–XVI, Elementary Engineering Treatment. 187 pages, illustrations, 20 × 27 cms. Schenectady, General Electric Review, 1938. Price $4.50.

Landmarks in Medicine, Laity Lectures of the New York Academy of Medicine, introduction by James Alexander Miller. 347 pages, illustrations, 14 × 20 cms. New York, D. Appleton-Century Company, Inc., 1939. Price $2.00.

Charcot of the Antarctic, by Marthe Oulie. 235 pages, illustrations, plates, 15 × 23 cms. New York, E. P. Dutton and Company Inc., 1939. Price $3.50.

Power-Factor Economics, by Price L. Rogers. 143 pages, illustrations, tables, 15 × 24 cms. New York, John Wiley & Sons, Inc., 1939. Price $2.50.

Photochemistry and the Mechanism of Chemical Reactions, by Gerhard K. Rollefson and Milton Burton. 445 pages, illustrations, tables, 16 × 23 cms. New York, Prentice-Hall, Inc., 1939. Price $5.75.

Sampling and Analysis of Carbon and Alloy Steels. Methods of the chemists of the subsidiary companies of the United States Steel Corporation as revised to 1937. New York, Reinhold Publishing Corporation, 1938. Price $4.50.

Some Episodes in the Manchester Association of Engineers, compiled by A. C. Dean. 139 pages, 15 × 22 cms. Manchester, Association, 1938.

A History of Science, Technology, and Philosophy in the Eighteenth Century, by A. Wolf. 814 pages, illustrations, plates, 16 × 25 cms. New York, The Macmillan Company, 1939. Price $8.00.

Engineering Progress, 1938. 40 pages, illustrations, 22 × 30 cms. Pittsburgh, Westinghouse Electric & Mfg. Co.

The Synthesis of Science and Religion, by Frederick Kettner. Biosophical Series No. IV. 22 pages, 15 × 22 cms. New York, The Biosophical Institute, 1939. Price 10 cents.

L'alluvion du Rhone valaisan Essai de determination de l'epaisseur par methode electrique, par Maurice Lugeon et Nicolas Oulianoff. 26 pages, illustrations, 17 × 24 cms. Lausanne, Imprimerie Commerciale, 1939.

Canada, Department of Mines and Resources. Improving the Properties of Clays and Shales, by J. G. Phillips. 39 pages, illustrations, plates, 16 × 25 cms. Ottawa, King's Printer, 1938. Price 25 cents.

Canada, Department of Trade and Commerce. Manufactures of the Non-Ferrous Metals in Canada, 1935 and 1936. 104 pages, 16 × 25 cms. Ottawa, King's Printer, 1939. Price 25 cents.

National Advisory Committee for Aeronautics. Technical Notes: No. 675, The Charging Process in a High-Speed, Single-Cylinder, Four-Stroke Engine, by Blake Reynolds, Harry Schecter, and E. S. Taylor. 58 pages, illustrations. No. 684, Experimental Study of Deformation and of Effective Width in Axially Loaded Sheet-Stringer Panels, by Walter Ramberg, Albert E. McPherson and Sam Levy. 110 pages, illustrations. No. 686, Local Instability of Symmetrical Rectangular Tubes under Axial Compression, by Eugene E. Lundquist. 25 pages, illustrations. No. 687, Loads Imposed on Intermediate Frames of Stiffened Shells, by Paul Kuhn. 44 pages, illustrations. No. 688, Comparative Performance of Engines Using a Carburetor, Manifold Injection, and Cylinder Injection, by Oscar W. Schey and J. Denny Clark. 21 pages, illustrations. No. 689, Tandem Air Propellers, by E. P. Lesley. 26 pages, illustrations. Six pamphlets, 20 × 26 cms. Washington, Committee, 1939.

CURRENT TOPICS.

The Research Laboratories of Mellon Institute.—H. S. COLEMAN in the Analytical Edition of *Industrial and Engineering Chemistry*, Vol. 10, p. 550, describes the elements of design, construction, and equipment of the research laboratories in the new Mellon Institute building, Pittsburgh, Pa. The institute's new home was erected entirely during the depression period from late in 1930 to early in 1937. Construction work was not pushed rapidly, and thus more than the usual time was available for study and perfection of the details of design and erection. A year's operation of the building has brought forth very few suggestions as to how the design of the rooms and equipment could be better suited to the purposes intended. There are eight floors, the four upper floors containing the typical laboratories, the fourth or main floor the administrative offices, library, and museum, and the three lower floors the auditorium, special laboratories, shops, unit plants, and mechanical equipment. Over one hundred laboratories are provided on the upper floors; about one-third of their total area has been left unfinished for typical or special laboratories to be constructed when and as needed. The institute's facilities are devoted primarily to basic researches in the fields of chemistry and chemical engineering. Its fellowship system, by which scientific investigations are conducted for a large number of companies concerned with widely varying types of problems, makes necessary the provision of many small laboratory rooms for the use of one or several fellows, whereas in a company research organization larger laboratories might be utilized advantageously and would have lower constructional cost.

R. H. O.

B.S. In Fire Protection Engineering.—An interesting commentary upon the profession of Fire Protection Engineering is afforded by the list of brothers who have studied for the degree since the inauguration, in 1903, of the course at Armour Institute of Technology, Chicago, Ill. It is due to Armour's unique position among technical colleges in maintaining, with the coöperation of Underwriters' Laboratories and the Western Actuarial Bureau, the only course leading to a B.S. in F.P.E. that such a list may be compiled. It is a testimonial to the inherent appeal of the profession of Fire Protection Engineering when brother succeeds brother, for the prospective student's knowledge of the field is a great deal broader

than that of the man who gains his knowledge from the somewhat roseate outline of a high school vocational adviser. The fact is emphasized through the wide range of class memberships; the disparities ranging from ten years to simultaneous attendance. A perusal of the list brings to mind the membership rolls of the old guilds of craftsmen and artists wherein the names of a family for several generations might be read. All of the trained minds guarding against loss of property and life throughout the nation have a bond in recognizing a common alma mater: Armour Institute. The exigencies of business may place the brothers at the opposite sides of the country, but the mutual feeling of kinship will extend beyond the common professional interest and serve to unite more firmly the Fire Protection Engineers in the common bond of service.

R. H. O.

Committee to Study the Effect of the Proposed New Definition of the Linear Units Now in Common Use in the United States.—In recent years there has arisen a demand for an international unit of length in those countries having industries engaged in making machine-tool parts or engaged in interchangeable manufacturing. In particular the lack of that uniformity in gage blocks, which can be obtained only through the use of a single standard, has been felt by such of our domestic industries as have been in competition with foreign industries. A practical solution has been found in some 15 countries, including the United States, by the unofficial adoption of a new ratio between the inch and the millimeter. The present ratio, contained in an Act of Congress July 28, 1866, is given by the following equation: 1 meter = 39.37 inches. In 1893 this relationship gave intrinsic value to the inch as a unit through the acquisition and official adoption of an international meter bar, which has continued and is intended to continue to serve as the primary standard of length in this country. Pending in the last session of congress was H. R. 8974, which, among other things, proposes, "That the United States inch shall be a legal unit of length defined as the distance comprising two-hundred-fifty-four-ten-thousandth parts (0.0254) of the distance defined as the meter." The meter is the basic unit, and therefore, the proposed change in the ratio would change slightly the adopted values of the United States customary units, making them shorter by about one part in 500,000. As a result of various protests an amendment to the Bill was suggested by the United States Coast and Geodetic Survey which would exempt from the application of the proposed law certain surveying and mapping activities. Professor Kissam, of the American Geo-

physical Union, Section of Geodesy, at the time of making a motion to consider the question, suggested a modification of the bill with respect to the United States Foot and the International Inch. The effect of the proposed legislation may appear in other fields of science or engineering which will make further amendment desirable for other purposes. It is doubtful if the small change in the ratio can directly affect any measurements except those carried out under carefully controlled laboratory conditions, but it will affect, by definition, computed or published data, tables, or formulas which have used the present ratio as a mathematically exact factor. Should any readers be desirous of contributing constructive comments, the Special Committee of the American Geophysical Union appointed to study this matter will appreciate hearing from them. The Chairman is R. M. Wilson, U. S. Geological Survey, Washington, D. C.

R. H. O.

New Applications of Sulphur.—W. W. DUECKER (*Mining and Metallurgy*, Vol. 19, No. 383.) The versatility, adaptability, and comparative cheapness of sulphur have stimulated endeavors to make it more useful for a greater number of purposes. In the ever widening field of plastics an organic polysulphide is now marketed under the name of Thiokol. This substance, resembling rubber in many characteristics, is comparatively inert to many organic solvents. This as well as similar olefine polysulphides, is soluble in and can be used to plasticize sulphur. With sulphur thus treated it is possible to make a variety of products which may be hard and resinous or definitely soft and pliable, depending on their polysulphide content. The use of plasticized sulphur with various aggregates forms a lute or cement. The product is low in moisture absorption or porosity, and inert to water, slightly alkaline solutions and the common acids. It shows extraordinary resistance to mechanical impact and abrasion and has admirable structural strength even when subjected to severe fluctuations in temperature. An investigation undertaken by Ohio State University to compare various jointing materials demonstrated that plasticized sulphur gives excellent results, the best conditions being obtained with a composition of a mixture of sulphur and asphalt. When a porous object is immersed in a bath of molten sulphur, new characteristics are imparted on cooling. Wood so treated increases in weight, strength and density. It is more resistant to moisture and some of the chemical inertness of sulphur is imparted to the wood. Preliminary studies indicate that sulphur may be used as the cementing agent in the centrifugal casting of pipe. Best results were obtained

when the sulphur was used in combination with sand and a rather coarse aggregate such as coke. Recently, citations have appeared on the application of sulphur as a circulating fluid in connection with the drilling of wells. Such fluids are used by drillers to cool the drilling bit, cushion the weight of the drill pipe, carry off the cuttings, and seal off water in the higher formations. Heretofore circulating fluids have consisted of muds, finely suspended barium sulphate or finely ground iron oxide.

R. H. O.

Microanalysis—A Tool of Modern Research.—E. B. ASHCRAFT (*The Electric Journal*, Vol. 35, No. 11). A group of new techniques is now available for investigating the structure and composition of materials. They open new fields to the analyst—and through him, to the engineer—making possible the solution of problems which only a few years ago would have presented almost insurmountable difficulties. In one case, the composition of an extremely minute sample—all that is available—may be satisfactorily determined. Again, a standard and routine procedure may be shortened until it is done in a half to a third the time. Or perhaps a means is developed for treating a complex mixture which could not be satisfactorily analyzed at all by ordinary methods. The new and powerful tool is microanalysis. There are numerous instances in which important and interesting results have been obtained by this method. In the electrical field an analysis was made of an unexpected deposit in the form of a small spot of a very thin film obtained in a vacuum apparatus. Again, a machine in service in a steel mill showed that a film was being formed consistently on the commutator. A bit of this film was scraped off and subjected to a microanalysis, giving conclusive evidence that poor commutation was caused by sulphurous gases in the atmosphere. In yet another case recently, a sample of dirt taken from the end windings of a generator was received in the microanalytical laboratory. Although the sample amounted to little more than a smudge on a piece of paper, it was possible to establish the nature and probable source of the dirt by microscopic and spectrographic examination. Furthermore a quantitative determination showed that something over a third of the sample was oil and grease absorbed in the dirt, thus confirming the engineer's suspicion of a leaky bearing. Such applications of microanalysis are constantly suggesting possible new methods of approach to the solution of many engineering problems, both old and new.

R. H. O.